PROCEEDINGS

Eighth Meeting, 58th Session

An Ordinary General Meeting of the Society was held at the Royal Society of Arts, on Thursday, February 1st, 1923, Professor L. Bairstow in the chair.

The Chairman, in opening the proceedings, said that Mr. G. S. Baker, O.B.E., of the National Physical Laboratory, would deal with flying boats and seaplanes. He would deal with the hull and its design, that part of the seaplane which differentiates it from the aeroplane. That subject had been touched on very lightly by Major Rennie at the previous meeting of the Society, in view of the present paper by Mr. Baker. Mr. Baker had begun work in 1912 on the problems of hull design, at a time when nothing of a definite nature was known; a few individual experiments had been carried out, but there was no systematised knowledge at all at that time. From that state of ignorance a great deal of experimental work had now rescued us. He did not know how far Mr. Baker would stress the point, but it was quite clear, from the investigation of certain accidents to seacraft, that there were fundamental differences in the behaviour of seaplane hulls on the water, differences which had a great deal of effect on the risk of flying. For instance, if one type of hull was such that when the plane rose in the air it stalled, then all the aerodynamical consequences of stalling followed, and there was difficulty. On the other hand, it appeared that we had a type of flying boat which did not make the plane stall on getting into the air, and consequently if it came back to the water it was still controlled. For this type of development, which he believed really dated back to the C.E.1, we were mainly indebted to Mr. Baker and his associates at the National Physical Laboratory, and to the generosity of Sir Alfred Yarrow in placing such a magnificent piece of apparatus as the experimental tank at the disposal of the nation.

Mr. Baker then read his paper on "Ten Years' Testing of Model Seaplanes."

"TEN YEARS' TESTING OF MODEL SEAPLANES"

BY G. S. BAKER, ESQ., O.B.E., M.INST.N.A.

Summary

§1. The Paper gives an account of the beginnings of tank tests of seaplane hulls carried out in the first place under the authority of the Advisory Committee for Aeronautics in 1912.

The tests commenced with plate experiments. The broad results of the trials of the floats designed after these first tests are given in §5. A detail account of the apparatus now used for standard tests (§7) is followed by some notes on the aims of the tests, and a very brief account of the work so far accomplished (§9). The differences between the model tests and the machine are dealt with in some detail (§11), and the method of estimating machine data from model tests is given in §12, together with the checks upon such estimates obtained by actual measurement on machines.

A list of published papers giving results of tests carried out in the tank, is contained in the Appendix.

Introduction

§2. For the benefit of the uninitiated, a brief account of the William Froude National Experiment Tank is given here so as to make clear what follows. The tank is a long concrete basin 550ft. in length and 30ft. in breadth, filled with water. A carriage on four wheels spans right across the water way. The apparatus used in testing a model is secured to this carriage, which can be propelled along the tank at any speed required for that particular test of the model. Speeds can be varied from nil to 25ft. per second, and towing forces up to 200lbs. can be measured without disturbing the steady movement of the carriage.

The tank was built mainly from funds provided by Sir Alfred Yarrow, so that research work in hydrodynamics, marine propulsion and naval architecture could be carried out in this country, and was mainly intended for the use of mercantile firms. It has been adapted for the purpose of seaplane testing at comparatively little expense. It should be understood that work for the mercantile marine has been carried out at the same time as the tests here described, and in increasing volume, and for this reason progress with aeronautic work is sometimes slow. Perhaps in the future it may be possible to build a really high-speed tank, devoted to this and high-speed vessel tests alone, when our data can be extended and work proceed continuously.

The majority of the work described is based on work carried out under instructions from the Advisory Committee for Aeronautics, the Air Ministry or other Government authorities. Recently a fair proportion of the test work has come more or less directly from firms. The remarks on full-scale machines are based almost entirely on data taken by the author or his assistant from observations when flying on various machines, and from the opinions and statements made by the various pilots on such machines. The Author is glad to take this opportunity of expressing his appreciation of the trouble taken by the pilots and the officers commanding air stations on his behalf in these tests, and of the work carried out by his assistant, Miss E. M. Keary, under whose care a good deal of the tank test work has been done.

Original Tests

§3. The first experiments on the shape of floats for seaplanes, of which reasonably full data is extant, were those carried out under the direction of Captain Sueter. These were made towards the end of 1911, on a full-size Avro machine in the Cavendish Dock, Barrow-in-Furness, and an account of them is given in Advisory Committee for Aeronautics Report T.157, dated December 21st, 1911. This report reached the tank through Sir R. T. Glazebrook, and after some discussion, I was asked by him to propose means and methods of obtaining data such as Captain Sueter mentioned in the report as being needed. It was due to Sir Richard Glazebrook's support and activity that this work was ultimately put in hand and advanced. In April, 1912, a B.E.2 machine fitted with a single main float was at work on the Fleet Pond, and I was able to see this machine, which did reasonably well on the water, put through certain tests, and a little later obtained some first-hand knowledge of the behaviour of the Avro, Farman and Short types of machines, and heard the opinions of Commander Samson and other expert pilots.

On May 10th proposals for tests in the tank were made to the Advisory Committee, and these were approved in June, 1912. At that time it was difficult to decide what qualities were desirable in a seaplane, and still more difficult to decide on a compromise which gave the best result. There were a few successful skimming motor boats in existence, the general features of which were known, and I had the advantage, in the autumn of 1912, of meeting Sir J. Thornycroft, seeing his models and discussing these with him and his daughter.

It was fairly clear that some of these motor boat features should be embodied in any seaplane float or hull, but that they would need some modification to allow of change of trim at high speed, and this was really the only starting point for experimental work.

§4. As the main features of these boats were flat planing bottoms for a fair proportion of the boat, associated in some cases with a seaworthy bow, it was decided to commence the tests by obtaining data relative to the lift and drift of a flat plane "skimming" on the water surface, and to follow this with tests on an elementary form of float. These tests were made on a surface varying in area up to 2.2 sq. ft. and in angle up to 10deg. with large areas, 15deg. with the smallest areas. The results are given in R. and M. No. 70 of the Advisory Committee for Aeronautics. The apparatus used is shown in Slide 1. It was necessarily strong and heavy, as the forces to be measured were large—ranging up to 70-80lbs.—and was modified for the float tests. The heavy gear for carrying the plate was replaced by a light carrier to which either one or a pair of floats in parallel could be attached. The apparatus did not allow of any change in running angle during any experiment, and this had to be set beforehand as required. The floats, however, could rise to their natural level at the fixed angle, this level varying of course with the speed.

Twenty-five models, beginning with toboggan forms and ending with types having flaring bows on motor boat lines, were tried in this set of experiments. The tests were necessarily incomplete in many respects because of their restrictions, partly because of the assumptions involved, and partly that we were exploring unknown ground, and the importance of some features was only realised slowly. The effect of gap between floats was tested at high speed and found to be negligible. We now know that gap, or rather want of reasonable gap, has its worst effect at 12-18 knots or thereabouts. The general effect of shape of bow upon the wetness of the floats, and their tendency to nose dive under certain conditions was, however, clear and instructive. The same remark applies to both the effect of ventilation holes to the step when this was fitted, and the effect of transverse shape of the bottom at the step. It should be mentioned here that for the sake of simplicity of construction, except in the case of one set of tests of concave and convex bottoms, the models were so shaped that any transverse section consisted of four straight lines, the sides flaring towards the fore end, these surfaces being "developable," *i.e.*, surfaces on which wide planks could be worked without "cockling," the top and bottom being horizontal.

§5. The experiments ended in an attempt to embody what had been learnt, in a design for a machine of some 2,000lbs. weight, the machine being designed at Farnborough, and the floats being arranged in general to suit their requirements. These floats were known as 43B, and are shown in R. and M. 70. Our tests showed a resistance for these only one-half of that of the original form, which was based on forms then built and in use. At that time there was considerable doubt in some minds as to the applicability of our results to the full-size machine. The method of testing differed in several respects from the actual conditions under which the machine worked, and the extension of data obtained with models some 20in. in length to floats 12 or 14ft. in length depended on theories, the applicability of which was considered as doubtful. These points are dealt with in detail in §12, and are mentioned here to explain the position at the time and the desire of the Tank and the Advisory Committee to test in practice what, from tank tests, was considered a good design of float. The lines of 43B were sent to Farnborough in November, 1912, and a little later to the Admiralty. Some trials of a machine fitted with these floats were made at Farnborough in July, 1913, with satisfactory results, but no direct comparison with other types was obtained. Such a comparison was at last made by the Admiralty at Calshot in April and June, 1914—the same floats being used. A machine of 2,200lbs. weight was used, the firm's original floats being removed and replaced by 43B

floats. The original floats were of toboggan type, the fore end having a rather quick contour to its bottom, and reduced beam compared with amidships. The 43B floats had flam bows with shallow mudgards to the step and pointed tails in plan. Lieutenant Longmore's report stated these floats were extremely efficient, the machine could get off the water with three-fourths instead of the full power required with the original floats, and the manœuvring power was good. The flatness of the contour of the mudguard caused solid water to pass over them in waves of 20-25ft. length when taxiing at 14 knots, and this did not look well. I was present on the machine in some of these tests, especially those made in the swell left by the Isle of Wight mail steamers. This broad confirmation of our results was satisfactory as far as it went, and there was no opportunity to obtain further checks for several years.

§6. The next step in the testing of seaplane hulls was taken towards the end of 1914. All the apparatus was lightened, and the floats were then arranged so that they would take up their natural running angle, and the load on the floats was applied at a point at the height of the centre of gravity of the complete machine, which was also the rocking and towing point. The floats as tested were a complete set for machine, *i.e.*, consisted of two main and one tail float in general or one single boat float. These changes led to greater accuracy in the data obtained, and brought the conditions of the experiments nearer to those of the full-size machine, especially at all low and moderate speeds. The first report of experiments with this apparatus was made in December, 1914, but was never printed. Report No. 165, of March, 1915, contains a brief reference to the gear, which continued in use with slight modifications (all tending to one end, viz., lightness of apparatus) until 1917. About this time another attempt was made to get a little nearer working conditions and to still further lighten the gear. A new apparatus was designed and made, and is shown in Fig. 1. This gear has been used for all experiments since 1917, and as it is still in use I should like to describe it in some detail.

§7. In the main it consists of a light parallel motion frame supported on the travelling carriage which spans the tank. On the rear end of the frame the model is hinged, so that it can rise and trim, but is constrained to run a straight course. The fore end of the frame is attached to a dynamometer for measuring pull. The hull or system of floats is usually stripped of all wings and tail air system. In detail the flying boat hull or system of floats is attached to a light framework, called the carrier. This carrier can rock about a horizontal transverse axis, on ball-bearings, the axis being situated relative to the models at the same height and longitudinal position, to scale, as is the C.G. of the complete machine to its floats or hull. Weights are so arranged on the float carrier that the centre of gravity of the whole is on this axis. The bearings on which the float carrier rocks are carried by a light transverse vertical frame, suspended on knife edges 30in. above the ball races. These knife edges are carried on steel rods which work in vertical guides secured to the towing carriage of the tank. A fine wire attached to the upper end of each steel rod, passes over a light pulley, suspended on enclosed double ball-bearings, and is attached to a scale pan. The weight of the vertical frame and float carrier is taken on these scale pans, and the load on or lift obtained from the water can be arranged by varying the weights on the pans. A towing eye can be attached to the float carrier at any height, corresponding to scale to the height of the propeller axis on the machine. The float and frame is towed from this eye by a horizontal gate, the fore end of which is carried on a transverse vertical frame suspended on knife edges fixed to the towing carriage, the distance between the two vertical frames being 9.0ft. The major portion of the weight of the horizontal gate is taken on this forward frame, which is balanced on its knife-edges when carrying this weight. The rear vertical frame is also balanced on its knife-edges with the float carrier and models in position, the models being sometimes in the air and sometimes immersed in the water at their full load line, according to the

experiments in hand. A balanced towing rod connects this forward vertical frame to the lower end of the carriage dynamometer, and the towing force is applied by weights on this dynamometer or by the spring attached to it, at its upper end.

When the towing carriage is moved down the tank the extension of this spring required to balance the float resistance is recorded on the drum. The speed at which an experiment is made is given by records also taken on this drum, one being time in half seconds and the other distance travelled in 20ft. units. The attitude or trim of the float and its vertical movement at any speed is indicated by the pointer which forms one arm of a light bell crank lever pivoted on the rear transverse frame, the other arm of the lever being actuated by a light vertical wooden rod secured at P (Fig. 1) to the model. The point P is so arranged that the line from it to the rocking centre of the float carrier is parallel to the horizontal arm of the bell crank lever. The vertical motion of the pointer is the same as that of the rocking centre of the float carrier, *i.e.*, the centre of gravity of the full-size machine, and its fore and aft motion gives the inclination of the float. These two movements are taken by an observer reading the position of pointer against a fixed scale on the carriage.

As the resistances to be measured vary downwards to about 0.4lb., mechanical resistance must be kept to a minimum, and all swinging frames are on knife-edges, and when making ordinary experiments the floats are kept free from contact with anything in the nature of control. The weights of all moving parts are measured separately in the shop after balancing, and a check on these is obtained when the whole is erected and balanced on the carriage.

§8. An ordinary or normal set of experiments consists in towing the model in this way in smooth water at a series of constant speeds from 10 knots upwards, varying the load with the speed to take account of the air lift (see par. 3, §11), until one of two things happens—either the model porpoises too much for the work to continue or the speed attained is sufficient to show the merits of the form, and is getting too high for the trim to be regarded as accurate without correction for the effect of air structure. In the latter case, if desired, a further set of experiments is made, at high speeds, keeping the speed constant and shifting weights forward and aft on the model carrier frame, and in some cases varying the load on the model to agree with the trim it takes up under the applied moment. In this way a cross curve showing the effect of any control trimming moment on the trim and resistance is obtained.

An observer watching such experiments will ask two questions, viz., What are the aims of the tests? and secondly, Is the data so obtained applicable to the machine itself? The latter question really raises two issues, the discrepancies between model and machine conditions, and the theoretical application and actual confirmation or otherwise on the machine of such data.

Aims of Tests

§9. These may be divided into two broad categories.

(*a*) The exploration of new fields by systematic work, and all research carried out with the object of defining the effect of particular features or supplying data for general design purposes. The testing of new ideas or modifications of old ones, and the reduction of proposed types of hulls on new lines to useful forms as regards water performance.

(*b*) The determination for all machines built, of those qualities of the hull which play a part in their general efficiency on the water, and the checking that small modification necessary to attain some other purpose, have not spoilt the performance on the water or rendered the machine dangerous in its behaviour.

Under the first heading may be gathered such completed work as tests on:—

The comparison of types and the determination of the broad lines for any type on which designs must be based. The loading up of hull, including, in the case of flying boats, the effect of varying beam with given displacement, and the effect of increased load on the water due to the machine running at small angles at high speed. Nose diving at low and taxi speeds, and a study of the wave making and water disturbance in general and of the features which lend themselves to a clean bow. Transverse stability of hulls at low speeds when ploughing, at high speeds when planing, and the effect of shape of transverse section of planing bottom, and of proximity of wing surface to the water on this stability. The effect of curvature and inclination of bottom within certain limits on resistance efficiency. The determination of the resultant forces on and separate purposes served by the planing bottom before the main step, and the portion of the hull abaft the main step. The forces involved in the general running and in the impact of a hull with the water at high speeds, measurements being taken of the total or resultant force when settling under various conditions, and of the actual distribution of the force over the planing bottom in smooth and rough water, when porpoising, getting off and settling. The longitudinal stability, the study of which has advanced sufficiently to enable the tank materially to reduce and sometimes completely eliminate any tendency to porpoise in smooth water. More recently the general effect of retractable and non-retractable wheels as regards resistance and when leaving or settling on the water has been investigated in connection with the development of the amphibian machine.

The data obtained in all these tests have been published at various times as reports and memoranda by the Advisory Committee for Aeronautics, and a list of these is given at the end of the Paper. In the early part of 1918 all of these reports were carefully examined, and a memorandum, R. and M. 437, issued defining what was required in a hull and giving guidance particulars and approximate data for securing them based on the experiments completed. This was brought up-to-date in a paper on flying boat hulls issued in 1920 under the auspices of the Institution of Engineers and Shipbuilders in Scotland.

It should be said here that all such data in existence (not having an obvious military bearing) is available for English designers and builders, and the staff has always been ready and willing to assist in its application to particular designs, and to give such help as may be possible to render such designs successful, or to overcome difficulties experienced when testing an actual machine.

Under the second heading comes the standard work with new machines, carried out as "normal" tests for firms or the Air Ministry, and much miscellaneous work. A normal report on such machines gives the following data:—

(*a*) The water resistance and running attitude of the machine for all speeds up to those when the hull is planing cleanly, and higher than this in the case of those hulls which are longitudinally stable.

(*b*) Ratio of load on water to hull resistance.

(*c*) The value of the air moments required at high speeds to overcome the water moments introduced if the machine is trimmed to angles out of the normal, the location of the trim for the minimum resistance, any important resistance change with angle of trim, and the determination of the maximum angle to which the machine can be trimmed without "rocking" it. By these tests the wing chord can afterwards be arranged for the machine to "get off" below the stalling angle without using heavy control moment, and it can be seen whether there is any inducement to run at large angles from a resistance point of view.

(*d*) Notes on its cleanness in running and its longitudinal stability on the water, and any special feature that may arise in the course of the work.

Modifications to overcome any difficulty are of course made in consultation with the designers, and data sufficient to define the effect of such changes are given.

FIG 2 RESULTS FOR FLYING BOAT HULL MODEL 353B

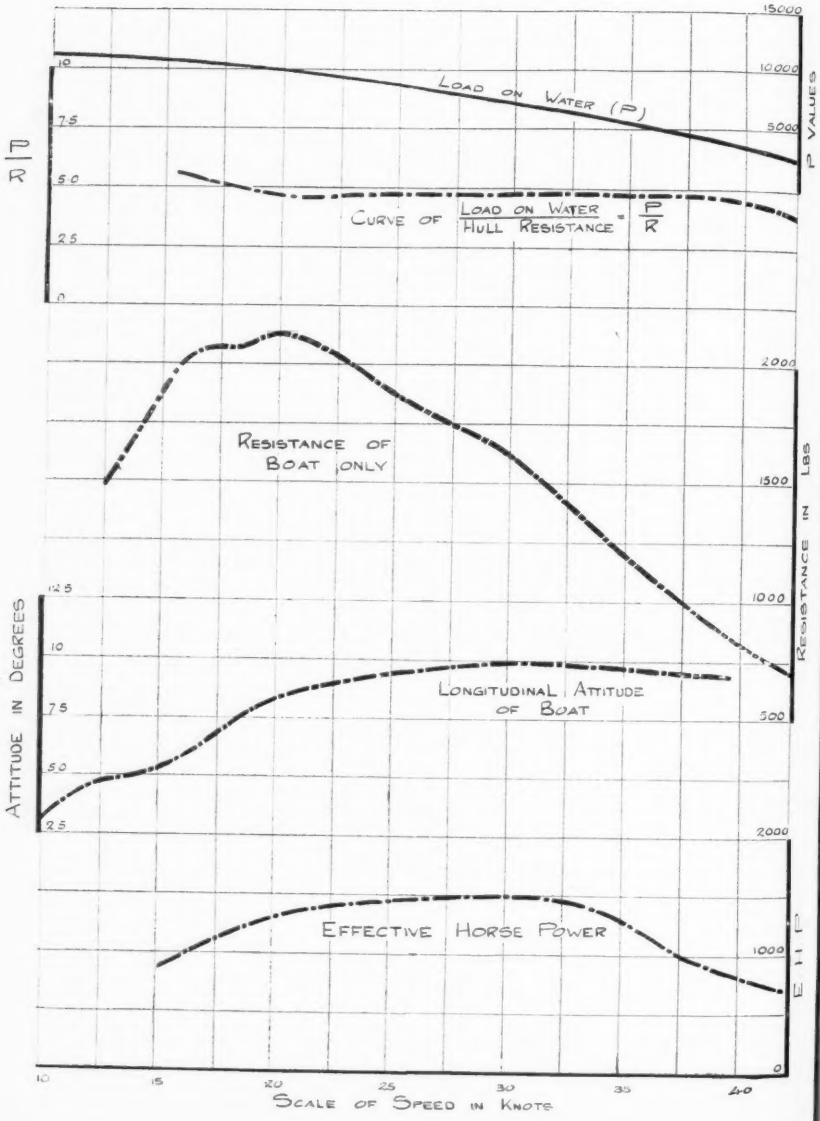
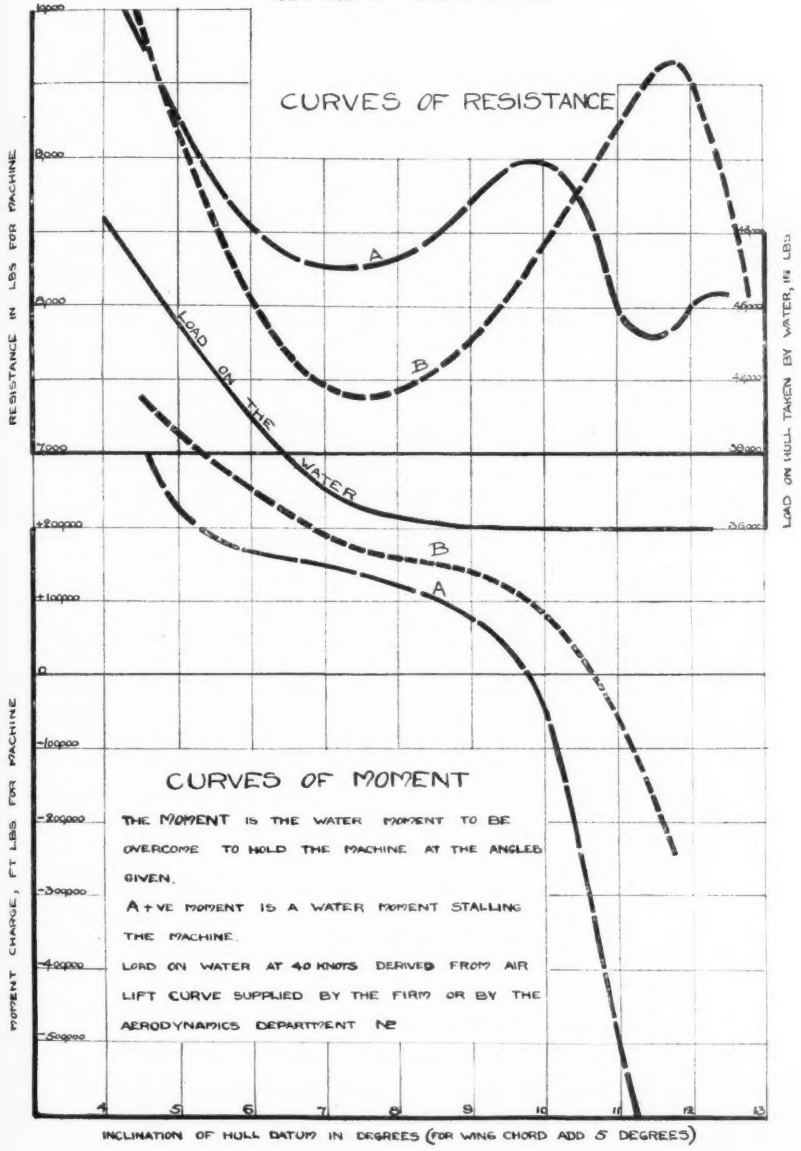


FIG 3 VARYING ANGLE OF HULL AT CONSTANT SPEED 40 KNOTS.

CURVES FOR HULL 84 FT LONG. DISPLACEMENT 100000 LBS AT REST GETTING OFF SPEED 50 KNOTS.


CURVES OF RESISTANCE
RESISTANCE IN LBS FOR MACHINE
LOAD ON HULL TAKEN BY WATER, IN LBS
A
B
LOAD ON THE WATER
B
A
CURVES OF MOMENT
THE MOMENT IS THE WATER MOMENT TO BE OVERCOME TO HOLD THE MACHINE AT THE ANGLES GIVEN.
A +VE MOMENT IS A WATER MOMENT STALLING THE MACHINE.
LOAD ON WATER AT 40 KNOTS DERIVED FROM AIR LIFT CURVE SUPPLIED BY THE FIRM OR BY THE AERODYNAMICS DEPARTMENT NE
MOMENT CHARGE, FT LBS FOR MACHINE
INCLINATION OF HULL DATUM IN DEGREES (FOR WING CHORD ADD 5 DEGREES)

§10. A typical set of diagrams is given in Figs. 2 and 3. It is hardly possible to quote in full a typical report, but some notes will serve to direct attention to the important features. The chief ones are :—

(*a*) Resistance and cleanness of running mainly at the centre half of the speed range, and in particular at the " hump " in the resistance curve. This hump was the cause of many of the early machines failing to get off the water with any load—the propeller could not exert a thrust sufficient to overcome it. With the high powers that can now be placed on these machines, this hump has lost its pre-eminent importance, and ranks along with cleanness of running (particularly with twin or quadruple propellers) and with

(*b*) Running angle in the upper half of the speed range and ability to trim back to an angle at which the machine will fly, when a flying speed has been attained. As the size of machines increases, there is an increasing need for running at an attitude which will give a reasonably low resistance without the use of any large percentage of the available control moment, and without it being *possible* to trim the hull so far back as to be unstable if it leaves the water. At the same time, in normal flight the axis of the hull should be in the direction of motion to keep its air resistance to a minimum.

(*c*) Longitudinal stability on the water. In the early days porpoising was always a troublesome feature, but now the form is altered if necessary so as to at least reduce it to controllable dimensions, and in many cases eliminate it in smooth water.

Application of Results

§11. With regard to the restrictions involved in the experiments, the assumptions made, and in what respects model conditions differ from those found in the flying of an actual machine. The main differences between model and machine are set out in Table I., and some comments on them are given below.

TABLE I.

DIFFERENCES BETWEEN MACHINE AND MODEL TESTS (§11).

FULL SIZE.	MODEL.
Screw thrust equals water resistance + air structure resistance + acceleration force.	Corresponding thrust equals water resistance + air resistance of hull and model structure.
Speed is increasing all the time.	Speed is steady.
Load on water varies with wind force and trim of hull.	Load on water is adjusted for any speed and true for one trim only.
Trim at all high speeds is determined by the air structure.	Trim of model at high speeds depends mainly on water forces.
Pitching is damped to some extent by the wings and tail system of planes.	No material damping against pitching.
Normally the water surface is broken into waves varying up to two or three feet in height.	Experiments usually made in smooth water, occasionally in rough.
Resistance at low speeds is augmented by the wing float being on the water.	Model tested upright without wing floats.

Screw and Model Thrust

The thrust developed by the screw is roughly constant at all low speeds, increasing somewhat as the flying speed is reached. This is balanced by the water resistance of hull, the resistance of the air structure and planes and by the product of mass and acceleration. In moderately still air the air resistance is small compared with the water resistance up to about .4, the flying speed, and in any case its line of action is not materially removed from that of the screw thrust. The mass and acceleration force acts at the centre of gravity of the machine, and as the propeller is always above the C.G. there is a nose diving moment present in the machine not represented in the model. Many experiments on the models have shown that such a moment has very little effect on trim and resistance until the speed has exceeded the planing speed by a fair margin. This difference therefore does not affect the applicability of model results up to this point. Well above this speed, if the model is free from "porpoising," experiments are made to determine the effect of trimming moment on running angle, and the actual running angle can be estimated from this data, or else best inclination determined for setting the wing chord relative to the floats.

Speed Variation

The steady acceleration of the machine involves a continuously changing streamline motion, and there is no corresponding variation in the model. The importance or otherwise of this difference depends upon the sensitivity of the streamline motion to speed change, the extent to which the water resistance is due to sheer impact with and dispersal of water, or to the streamline motion set up. There is no data extant which enables one to come to conclusions on these points. A considerable portion of the resistance is known to be due to mere impact with and dispersal of the water, even at quite moderate speeds, and generally the flow *under* the model changes only slowly with speed. When the machine has reached a speed at which it can plane, this difference probably causes a little change in the speed at which planing occurs in model and machine or in machine when getting off and settling, but otherwise there is some reason for supposing that error due to this cause is not large, but only comparison with machine would determine the point.

Load on Water

The load in the machine is the total weight less the lift on the wings, which varies with the attitude of the machine. On the model this air lift, if data is not supplied by the designer, is obtained by taking it to be the total weight when the speed over the water is the "getting off" speed, and that at other speeds it varies as the speed squared. As the machine nearly always gets off at an angle fairly close to the maximum lift angle, this really comes to assuming the air lift is the maximum at all speeds. The attitude of the machine varies during acceleration from 4deg. to 6deg. at rest to the maximum lift angle at about 17 knots, remains at this angle or above up to about 25 knots, and then is more or less under the control of the pilot and is usually kept a little below the maximum lift angle until he is ready to get off. Fortunately, at speeds below 17 knots the air lift is very small in still air and light breezes, so that error due to the loading is small until the machine is well over the resistance hump. Above this speed the load in some cases is varied with the attitude of the hull to take account of the varying loading of the wings.

Trim

The trim of the machine cannot be altered by any available control until the machine is planing, model and machine running at the same angle. But at high speeds, the forces on the air structure have a greater effect than the water

forces, and the pilot can vary the running angle. It is for this reason that the cross curve at constant high speed already mentioned is made.

Pitching

Compared with the machine, the inertia of the model in pitching is large. If, therefore, any pitching develops in the model, it will come on relatively slower in the model, but will get worse than in the machine owing to the entire absence of any damping of the oscillations from the wing and tail surfaces present in the machine, the general result being the exaggeration of porpoising in an unstable form.

Water Surface

All model tests are made in smooth or nearly smooth water, whereas the machine is run under ordinary sea conditions. Smooth water has always been used, as it is considered the only safe and repeatable criterion of performance of the different forms submitted to tests. Experiments to test seaworthiness are sometimes made in either a regular or irregular sea, but resistance tests are rarely made under these conditions.

Wing Float Effect

If wing floats were fitted to the model it would add to the expense involved and render comparison of the main hulls more difficult. Their resistance can be separately calculated when necessary. They would normally be out of water as soon as the machine reached the planing condition, as its transverse stability on the water is then usually sufficient to keep it upright.

Estimates for Machine

§12. In predicting results for machine it is assumed that moments, forces, dimensions and speeds vary as the fourth, third, first and half power of the relative scales, so that if M.P.L.V. are these quantities for model $1/n$th full size, the corresponding quantities for the machine will be Mn^4, Pn^3, Ln and $Vn^{\frac{1}{2}}$ respectively. Angles will, of course, be the same for machine as in model. The above procedure neglects all corrections for any possible differences in relative value of the skin friction resistance of the model and machine. Experiments with two models of a very efficient type of motor boat, one 13 feet in length and the other 4 feet in length, showed that the skin friction correction required to bring the two sets of data into agreement at high speeds was quite small. For this 40ft. motor boat a little over the hump speed the total skin friction correction was 9 per cent. of the measured resistance. Observations of the area of bottom in contact with the water showed that this diminished with speed and the correction required would also diminish. A certain amount of other experimental data supports this inference. It is believed, therefore, that the present procedure is correct at high speeds, errs a little in giving somewhat high powers at low speeds, and that the moments given for the tests at high speeds are about correct as they are given. Any error involved is probably balanced by adverse sea and weather conditions with the machine.

§13. A more searching check upon the accuracy of estimates based on model results, is to measure the corresponding data upon full size machines, but there are many difficulties, mechanical and otherwise, in the way of such work. In the course of the past ten years the author and his assistant have flown on a number of machines, always with the object of getting such data when feasible. But it is difficult to obtain facilities for this work, and the data obtained, without considerable preparation and some expense, is small in individual cases. The most important tests of this kind were those carried out at Grain in 1918 with a C.E.1 machine. In these experiments the machine was first towed by a destroyer at

various speeds up to 20.2 knots, the speed resistance and running angle being measured. The machine was then run under its own power at steady speeds, ranging up to 25.0 knots, and the running angle and data *re* the clearing of the steps and commencement of planing obtained. A comparison between the measurements so obtained and the estimate from the model is given in Table 2.

TABLE 2.

COMPARISON OF MACHINE AND MODEL.
C.E.I. EXPERIMENTS.

Machine Speed. Knots.	Resistance Estimated. (Lbs.) Water only.	Air.	Total.	Thrust in lbs. Propeller calculated from revs. on taxi trials.	Actual measured pull. (Lbs.)	Wing Chord Angle in Degrees. From Model.	Measured.*
9.3	455	432	487	532	560	11.3	11.4
11.5	520	51	571	624	640	12.3	13.1
12.6	—	—	—	—	—	12.4	12.1
14.5	678	83	761	696	670	12.7	12.7
15.2	705	100	805	710	760	13.0	14.6
17.0	678	133	811	—	850	14.5	14.7
17.5	674	153	827	762	880	14.7	16.1
19.3	680	186	866	—	870	15.0	15.9
20.2	679	205	884	—	870	15.1	15.8
24.8	—	—	—	—	—	15.1	14.2

* In all trials but two the tow point was below the propeller centre, and the forward trimming effect of the tow was a little less than it should be. In the runs at 12.6 and 24.8 knots the machine was self-propelled and the trim a little less.

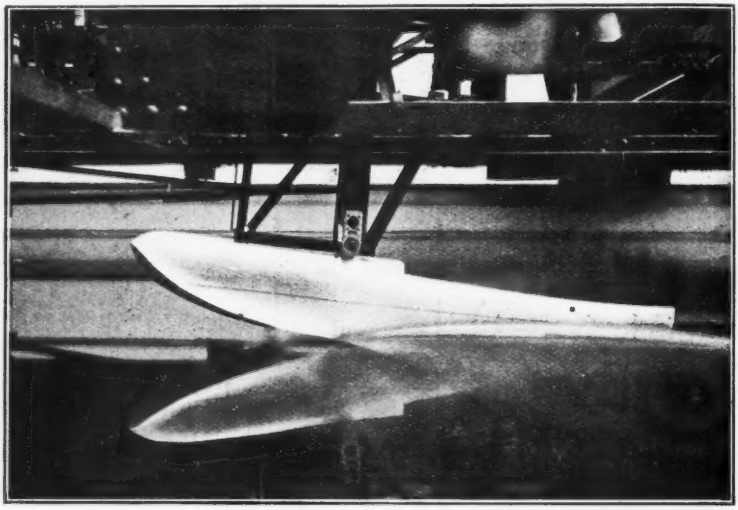
The agreement as regards resistance angle and planing was considered satisfactory over the speed range of the tests. A similarly close agreement between running angle and commencement of planing has been obtained with a P5 flying boat. Thanks to the facilities provided by the Supermarine Aviation Company, comparison of the stability on the water (both transverse and longitudinal) of their four-seater channel flying boat was rendered possible, and as far as could be seen there was general agreement. Speeds had to be taken on the machine's instruments which were stated to be fairly accurate. Predictions from the model tests of failure to fly have been confirmed by two machines built, and the predicted possibility of flying only in favourable conditions in one machine was also confirmed. But such confirmation is necessarily not quantitative, but general in its character.

The only discrepancies so far found are in running angle at high speeds. Both in the C.E.1 and in the P5 this angle was smaller by some 3 degrees than in the model. It is now recognised that this is due to the fact that the air structure has a large effect on the running angle at high speeds, and our cross curves at constant speed with varying angle are made to deal with this feature. It is not pretended that other discrepancies do not exist between model and machine performance. I am always anxious to obtain details of any trouble in a machine and, if possible, to trace it to its source. One's very real difficulty in such matters is to get the real facts separated from some observer's opinion. Most pilots' observations are reliable, but their conclusions as to causes necessarily require revision in the light of better technical knowledge.

Future Tests

§14. In the lecture so far the author has tried to show how the experiments in the past have grown in purpose and usefulness, and slowly taken certain more

or less standard shapes. It is to be assumed that in the future these methods will grow and change with the types of the machines, and one's endeavour in test work is to keep always a little ahead of such changes, so that the test data serve as a guide to the designer. We are beginning to regard data from two points of view, the small machine and the really big one, leaving intermediate ones for individual consideration. Ten years ago our first research paper dealt with the machines then in use of weight less than one ton. Our latest general research paper covered machines of the single hull type weighing 50 tons. Our other work has dealt with machines mostly of 2 to 5 tons, occasionally of 15 tons, generally of amphibian character, and these for the most part indicate the drift of design. Qualities which are easily embodied in a small machine become more difficult of attainment in the large, and in some cases more essential. The bigger the machine the greater is the accuracy required in the design, and in such machines model tests of all features are imperative. In the small ones, small



Flying boat hull (Supermarine Co. design) under test at speed 30 knots for machine.

errors in design are easily eliminated, often at less expense than is involved in making tank tests. But new types, especially of amphibian machines, should be tank tested. Special tests at high speeds are now made with these, for trimming when settling on the water, to determine the trimming effect of the wheels.

Whether the next developments will follow the two general lines of small amphibian and large flying boat cannot be said with any certainty. In the author's opinion the really large flying boat is coming. The direct trade routes from this country to Scandinavia, the ports of the Baltic Sea, Germany and to America are almost entirely by water, and all our Colonies are separated from us by great oceans. To carry a large number of passengers large machines are required, and any machine not capable of settling on the water would hardly come up to accepted standards of safety on these routes. Research on such lines should not be deferred until the machines are here, but should be well in advance of the present requirements and should be unrestricted in its general character.

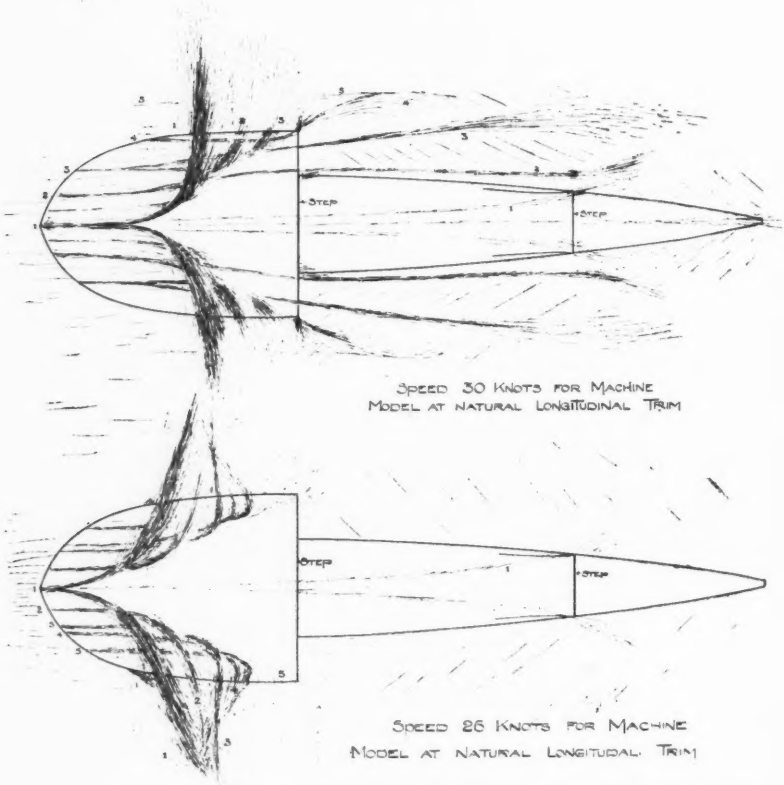
APPENDIX I.

PUBLISHED PAPERS GIVING RESULTS OF TESTS CARRIED OUT IN THE WILLIAM FROUDE NATIONAL TANK.

Date of Publication.	Subject Matter.	Form of Publication.
November, 1912	Plate results, early forms up to model 43. Air tests on 43b.	R. & M. 70
November, 1913	Twin floats, effect of tail angle, ventilation of step, gap at high speeds. Single float of great beam, with and without bastard tunnel.	R. & M. 98
July, 1914	Float type, general requirements, etc.	Trans. I.N.A.
July, 1914	Balloon shape floats, with and without steps.	R. & M. 113
March, 1915	Twin floats—effect of shape, chine, step flare, etc. These were the first tests with a complete set of floats and deals with trimming moment for first time. Longmore's report on 43b floats.	R. & M. 165
June, 1915	America type tests and some two-step flying boats. Effect of beam on resistance.	R. & M. 166
November, 1915	America type, effect of beam.	R. & M. 187
December, 1915	Transverse stability on water. Effect of cross section on same. Notes on wing floats.	R. & M. 188
January, 1916	Twin floats, comparison of types.	R. & M. 189
November, 1916	Flying boats with hooped sections. Effect of size for constant load. Effect of beam for constant load.	R. & M. New Series 300
November, 1917	Wave formation produced by a seaplane.	R. & M. New Series 365
April, 1918	Tests leading to C.E.1 design.	R. & M. New Series 412
March, 1918	Tests for stable form of float for launching air target.	R. & M. New Series 410
May, 1918	General summary of results for flying boats, deals with bending moments on hull for first time.	R. & M. New Series 437
September, 1918	N4 type, some notes on wing floats for same.	R. & M. New Series 472
December, 1918	Comparison of F5, Vickers' F5 and P5 forms for stability and resistance.	R. & M. New Series 483
March, 1919	Impact forces on hull when settling. Total force measured under varying conditions.	R. & M. New Series 583
January, 1920	Loading of flying boat hull, effect of beam and of angling up bow.	R. & M. New Series 655
February, 1920	Flying boats. General data on effect of dimensions, details of form for good results and some strength data.	Trans. N.E.C.I. Eng. & Ship.

EXPERIMENTS WITH FULL SIZE MACHINES.

September, 1918	Summary of early full-scale work. Tests of C.E.1 machine, for longitudinal stability, resistance and inclination.	R. & M. 473
September, 1920	Impact forces on hull when taxi-ing and settling, amount and locality.	R. & M. 683



GENERAL FLOW UNDER HULL OF FLYING BOAT.

The diagram shows the flow of coloured water under the hull, the streamline having the same number entering and leaving. It will be seen that the thick front edge of the water thrown out is produced first by the forefoot of the hull and aggravated if the water cannot easily flow under the lip of the chine at the step (see return of line 4 in lower fig.).

Discussion

Wing Commander T. R. Cave-Browne-Cave, who opened the discussion, said there could be no possible doubt as to the value of tank work. Not only were the actual tests carried out in the tank of very great value, but the knowledge which Mr. Baker and his staff had acquired in the course of those tests made them of extraordinary value when dealing with difficulties occurring in full-scale boats. There were one or two points he would like to raise, which were rather in the nature of requests for further information than criticisms of the work done or of the lecture. When Mr. Baker was comparing results of model and full-scale tests, he had referred to the forces necessary to accelerate machines and it did not appear in the model. He did not know whether there were any calculations that would account for that difference, whether they could use the mass of the machine, or whether there was a certain virtual mass which must also be accounted for. He believed the practical limitation of the flying boat was reached, not so much in its ability to get off the water under calm conditions, but under certain conditions of sea, of which he thought the worst was the long flat swell with very little wind. He had seen the apparatus available in the tank and could not help thinking that a great deal could be done if models were tested in such a long low swell, because the occasions on which a flying boat failed to function were much more frequent at the time of a long difficult swell than when the boat was loaded to the maximum and was trying to get off in a perfectly flat calm or, alternatively, when there was the bigger sea which was accompanied by a fairly strong wind. It would perhaps be well if the design of the boat were made specially suitable for that difficult condition rather than for the rather less difficult condition of a perfectly flat surface. He had been extraordinarily interested in the diagrams shown of the flow of water under the surface of the boat, and it would be valuable if Mr. Baker would add to the text of the paper a sufficient description to allow those diagrams to be published. As the size of flying boats increased, and as the power of the machines increased, one of the biggest difficulties was to accommodate the propeller or propellers. The difficulty was that the water was thrown up by the boat as it passed along and then broke up and passed into the propeller. The density of water was some 900 times the density of air, so that even a comparatively small quantity of water in the air meeting the propeller might very well put up the loading by ten or more times. Therefore the difficulty of the propeller was a very serious one indeed. Perhaps the most marked respect in which the model did not represent the full scale was in the way in which the water was thrown up. He quite believed the flow under the bottom of the model was the same as the flow under the bottom of the full size machine. The law of similarity established that without doubt, but he very much doubted whether the law of similarity held in the motion of this thin film of water through the air. There were present at the meeting two of the greatest authorities on aerodynamics, namely, the Chairman and Mr. Baker, and he would be glad if one or both would assist him in that respect. He believed that what happened was that the water was thrown off from the hull in the same way as it was thrown off by the model, but in the manner in which the water travelled after it was thrown off there seemed to be a large difference as between the model and the full-scale machine. The motion of the water after it was thrown off was a most serious matter, because that was responsible for breaking the propellers, and the avoidance of the tendency to break the propellers was a very serious restriction on design. He did not know whether it was possible to state the way in which the water travelled after it was thrown off. There was another, but quite different, point arising out of that. The Americans had employed motor boats, which he believed they referred to as sea sledges, which were provided with a V-bottom, but with the V upside down. Such a shape would appear to throw much less water to the sides and, consequently, less water on to the propellers, and if that shape worked out satisfactorily from the aero-

dynamic point of view it might have certain very valuable characteristics. The question of the seaworthiness of the flying boat was a most difficult one, and whether seaworthiness would improve as size increased was, of course, an important problem. Whether it was possible to investigate that with models he did not know, but he believed that the principal justification for the large flying boat was the hope that seaworthiness would improve. He did not think the performance went up very greatly with increase of size. There was one point on which he did not agree with the lecturer, although he admitted that he was prejudiced. Mr. Baker had predicted trans-oceanic journeys by flying boat, but he himself was going to be comfortable and travel by airship.

Sir RICHARD GLAZEBROOK, K.C.B., F.R.S. (late Director, National Physical Laboratory), expressed his appreciation of the paper and called attention to the progress made since 1912, when the possibility of making tests on floats of seaplanes and so on in the tank was first spoken of. Mr. Baker had pointed out very clearly and very distinctly in the paper the method adopted in trying to secure that progress and the results which he had attained. Mr. Baker had also made it very clear that further research on these matters, as, indeed, on all matters connected with aeronautics, was very greatly wanted. In the forthcoming year it might be possible to put rather more funds than had been put hitherto at the disposal of those interested in working the tank, and already a small panel of experts had been appointed to help Mr. Baker and discuss with him the problems arising. He believed we could look forward to further progress and that we should be able to congratulate Mr. Baker again, as on this occasion, at a future period, on successfully carrying forward the work. He would like to direct special attention to that part of the paper in which Mr. Baker had pointed out the importance of getting forward with research work. Mr. Baker had said, "Research on such lines should not be deferred until the machines are here, but should be well in advance of the present requirements and should be unrestricted in its general character." We did want, said Sir Richard, to have research unrestricted in its general character in order to investigate the very difficult problems arising in seaplane work. There were many points on which possibly something might be done. We had learned much in connection with ordinary machines through careful investigation of the laws regulating the flow of air around the machines, stability in the air and so on, and we wanted the same kind of knowledge in regard to seaplanes, and in order to secure that it was necessary to continue the co-operation that there had been in the past between those working on the tank and those carrying out full-scale tests in the air.

Captain D. NICHOLSON said that the Society could congratulate itself on having a paper from Mr. Baker, who was not only the expert on tank experiments in this country but in every other country. Being himself a naval architect, he fully realised the difficulties Mr. Baker had to face. Referring to his experiences in connection with one of the early tanks on the Clyde, about twenty years ago, he said that at that time very many shipbuilders worked either by past experience or by rule of thumb, but when they had found that other shipbuilders were able to reduce the cost of boats by using the tank, the tank became most prominent. Many shipbuilders were now realising that the tank was of great value, not only from the point of view of the yacht, but from the point of view of the high speed motor boat. Mr. Baker had said that pilots' observations were very often correct and very good. He agreed that they were, but when they asked pilots to give some explanation as to how particular defects might be remedied, as a rule, he did not think it was fair to expect them, with the technical knowledge they had available, to say how it ought to be done. It should be left to technical experts to give the remedy. He agreed with Mr. Baker's suggestion that the larger flying boats were really coming. He did not know exactly how Mr. Baker would answer Commander Cave-Browne-Cave with regard to seaworthiness, but he him-

self believed there could be no doubt about seaworthiness improving as the boats increased in size. As a matter of fact, he had had a great deal of experience with hydroplanes and motor boats, and seaplanes on similar lines to flying boats had greatly increased in seaworthiness as they had increased in size. Mr. Baker had not mentioned the materials of which the models were made. So far as he knew, all the models of flying boats and seaplanes had been made of wood. He could not understand why they should not be made of wax, because if defects were found in the lines of the boat it took a great deal of time and was very difficult in some cases to alter the lines of a wooden model. If they were made of wax it would be much better, because little pieces could be taken off or added on much more easily. Again, he would like to know whether Mr. Baker had carried out tank tests with the model completely submerged. He believed the Americans had been doing that for the last two years. He was glad to see that the Air Ministry had been carrying out many more tests in the tank recently than they had done previously; they were beginning to appreciate that the tank was of some use, and he would like to see a small boat given to the tank so that experience could be given direct from the tank to the D. of R.

Colonel DE VILLAMIL asked if the longitudinal curve on the bottom of the boat was based on any principle; was it a mathematical curve or only a streamline, because they never knew whether a streamline was too much or too little? He was lately at the Model Exhibition, where a young fellow had a boat, at the bottom part of which he had made a parabola, and he had said that the parabola gave very much the best results. The parabola, of course, was a curve of uniform acceleration and also, he believed, of least resistance.

The CHAIRMAN laid emphasis on the need for facilities for full-scale work. Not only in regard to seaplanes, but in regard to every type of aircraft, he believed we could say that we were further ahead in our knowledge of models than on the full-scale machines. It was extremely necessary that a limited number of full-scale experiments should be carried out, although they were relatively expensive. The whole problem, looked at from first principles, was so complicated that we could not hope, in the near future at any rate, to solve it from that point of view. From a purely experimental point of view model experiments could be restricted in number and type, with a considerable saving in expense and time, if we knew roughly what happened on a full-scale machine. In nearly every case, not only in regard to seaplanes on the water, but in regard to all heavier-than-air craft in the air, as well as airships, it was found that the model approximated very closely to what happened on full-scale machines. Another point connected with cost was that he believed less than £500 per annum, on the average, had been spent on model tests, and yet Mr. Baker had said that it was already predicted that two types would be certain failures, and that a third would be a partial and almost complete failure. There was now something less than £30,000 per annum available for model work. The economy to be effected by conducting experiments with models first, particularly if we could get able experimenters such as Mr. Baker, with adequate facilities and, still more important, suitable conditions of working, *i.e.*, a chance to do what he could see was necessary was very great, but we in this country, and possibly also those in America, had reached the stage where the speed at which we could move forward depended to a large extent on getting further facilities for full-scale experiments. He wished our prospects in this country were as bright for the continuation of those experiments as they were for the continuation of model experiments. Commander Cave-Browne-Cave had mentioned the breaking up of the film of water at the side of the boat. He would not attempt a full reply at the moment, but it was extremely likely that the main cause of it was surface tension. If they assumed that it was surface tension the law of comparison for scale could be worked out, but he had not attempted to do it. He then called upon Mr. Baker to reply to the discussion.

Mr. BAKER, replying, first thanked the meeting for their kindly reception of his lecture. He had not set out to do quite what Commander Cave-Browne-Cave had apparently expected, namely, to give a lecture on the various features that ought to be embodied in the hull of a flying boat. His object was to give an account of what had been done in the tank and how they had set about doing it, the experimental differences between model and full-scale work, and to throw what light he could on the effect of those differences. For example, with regard to the acceleration force, Commander Cave-Browne-Cave had asked if he (Mr. Baker) knew what mass to use in working out the force which was equivalent to the acceleration. He could only say that he did not know the virtual mass and he had never seen it worked out by anyone. With regard to the effect of a long flat swell, he believed that problem could be tackled at least mathematically, and that he and his colleagues could tackle it experimentally with the aid of the wave-making apparatus they had at the tank. They could make, in the tank, such a long low swell as that to which Commander Cave-Browne-Cave had referred and could run models in it, but he considered this was a problem which could be treated in the same way that Mr. Kent* had recently tackled the question with regard to the pitching of ships, *i.e.*, from the mathematical point of view first. He would have much pleasure in adding one more diagram to the paper if the Society wished, showing the flow of water underneath the bottom of the boat, together with a few notes on the subject to explain what was shown, and possibly to go a little further than that by giving some general idea of what it meant in regard to the design of the hull. With regard to the spray thrown up in the model and in the machine, there were two factors in this, namely, surface tension, as mentioned by the Chairman, and the wind. In the tank, of course, there was no wind at all, but the machines invariably sailed into the wind, which would blow to pieces the thin film which was thrown up by the model and leave the thick front edge of the wave, and its rear portion where the wave hits the water practically unaltered. That solid piece of water, so far as he knew, would be the same in model and full size; there was no reason why it should not be so. The difference would come about when it got thin enough to be affected by surface tension or wind. Commander Cave-Browne-Cave had said that he held the opinion that the spray thrown up by the model was not representative of the spray thrown up by the machine, but he had not given any actual facts. He (Mr. Baker) had been on a number of machines and had watched this—it had not been possible to measure—and he had never seen any material difference. He had watched when the step was clear, the speed at which it occurred, and had observed the same thing on the model, and it was the same. The angle of the model and the machine was the same, and until someone produced definite data to show that the two things differed, he preferred to hold the opinion he had expressed, which was based, at any rate, on theoretical reasons. Commander Cave-Browne-Cave had also mentioned the American sea sledges, but the Americans were not the first with that type of boat. He had seen something like them 15 years ago, working at Spithead. They were not called sea sledges then, but the "Viper" type of motor boat. They had the hollow underneath in the centre of the hull, as in the case of the American boats. Experiments were being conducted on that type of machine at the present moment, and there was no doubt that they were cleaner with regard to the wing of water which was thrown out by the ordinary type of flying boat hull. Beyond that he did not think it would be very wise to say much at the moment. The experiments would be extended to see whether it was the proper type of hull to put on a flying boat with safety to the people in it. As to the remarks of Sir Richard Glazebrook, nothing had pleased him (Mr. Baker) more than when he had heard that there was to be a Seaplane Panel of the Advisory Committee for Aeronautics. He hoped that through that panel we should be able to make definite progress with the problems that required to be

* T. Inst. N.A., 1922.

tackled, and that that progress would be continuous, so that people could be kept continually at work on research. The last twelve months had been rather painful in that respect; in the first six months everybody had had to be taken off the work and new work found for them, and that sort of thing did not make for efficiency or for the furtherance of our knowledge of seaplanes. It was unnecessary to say that more research was necessary, but research could not be carried on without money; expert researchers were necessary, which meant a continuous flow of money, and that was all that was required so far as he and his colleagues were concerned to carry on with their work. On the other side of it, the comparison of their work with the full-scale machines needed the co-operation of the flying stations. They were fortunate in having had, first, Commander Bustead, and secondly, Commander Cave-Browne-Cave, at Grain, and more recently Commander Cave-Browne-Cave had on his staff Mr. Evans, who, he felt sure, would help a great deal on the technical side, in comparing full-scale work with model work. He hoped, therefore, to see something more like continuous co-operation instead of the necessarily spasmodic co-operation that had existed hitherto. Captain Nicholson had asked why the models were made of wood and not wax, owing to the fact that the latter could be altered more easily. The wood was lighter than the wax if the models were solid, and it also stood the comparatively hard knocking about which the model received during test. They could take its weight off the water on pulleys, which had practically no friction, and except that the rather heavy weight of the wood was a definite defect as regards inertia, and particularly in the porpoising problem, he saw no reason to change. Completely submerged models had been run in the tank on two or three occasions, and they had compared the results with the results of models in air. Dr. Stanton had also carried out experiments in the tank on the same subject with airship forms, and the results were reasonably alike and made one think it was fairly certain that when we got a direct and proper comparison in that way they would find the results alike. He hoped to say more about it twelve months hence, after Mr. Southwell and himself had completed an investigation which had been commenced, using the same apparatus for the comparison of models in the water and in the air channel. Colonel de Villamil had asked whether the longitudinal curve given to the bottom of the boat was based on any mathematical formula. At one time he had produced a formula for getting the curvature of the bottom of the float, but like most other sensible people he had let it go and went for what experiments had shown was necessary rather than for what mathematics had shown might be necessary. He knew of no one at present who drew out the forms of flying boats from mathematical formulæ. A similar formula did exist for ship shape forms and was used in America occasionally, but he did not think it was ever used in England. Coming back to the need for experiments on full-size machines, in order to check and guide us in regard to what was being done with models, nobody knew more than he did how difficult those full-scale experiments were to carry out, but that it was really necessary that they should be carried out was a foregone conclusion. It was necessary to have a knowledge of the general behaviour of a machine when planning out experiments on the model, and when they were working them out in detail they must be able occasionally to compare the results with those obtained on the full-size machine.

A vote of thanks was accorded the author at the conclusion of the discussion.

LEONARDO DA VINCI AS A PIONEER OF AVIATION

BY IVOR B. HART, B.SC., ASSOCIATE FELLOW.

Introduction

Leonardo da Vinci lived in the fifteenth and early sixteenth centuries (1452-1519). He was the natural son of Ser Piero da Vinci, a Florentine notary, and at the age of fourteen was apprenticed to Andrea Verrocchio, a famous artist of those days. Verrocchio's tastes, and as a consequence his circle of acquaintances, were wide, and from all these Leonardo imbibed and developed a passion for scientific inquiry side by side with his development as an artist. At Florence in his early days he came under the influence of such men of science as Benedetto del'Abbaco, Giovanni Agiropulo, L. B. Alberti and Toscanelli.

In 1483 Leonardo migrated to Milan, where he took service under Ludovico Sforza in the capacity of consulting engineer, architect and sculptor, and he was busily employed in all these capacities. His chief scientific friendship during this period was with Fra Luca Pacioli, the famous mathematician.

Leonardo's stay in Milan ended in 1499 with the collapse of the power of Ludovico Sforza, and for some years we find da Vinci back again at Florence. In 1506, however, he accepted an invitation from Louis XII. of France to return to Milan. He remained there till 1512, and later, in 1515, Francis I. of France, Louis XII.'s successor, invited him to take up his residence in the Castle of St. Cloud, near Amboise. Here he spent the remainder of his days. He died on May 2nd, 1519.

His chief writings on the subject of flight are to be found in the following three publications:—

1. "Manoscritti di Leonardo da Vinci—Codice sul Volo Degli Uccelli, e Varie Altre Materie."—Publicato da Teodoro Sabachnikoff—Paris, 1893.
2. "Les Manuscrits de Leonardo da Vinci. Les Manuscrits B.F.K. & L. de la Bibliotheque de l'Institut de France."—Par M. Charles Ravaisson-Mollien—Paris, 1883 to 1893.
3. "Il Codice Atlantico di Leonardo da Vinci nella Bibliotheca Ambrosiano di Milano reprodotto e publicato dalla regia Academia dei Lincei sotto gli auspici e col sussidio del Re e del Governo."—Giovanni Piumati.

1 *Flight Before Leonardo*

Aeronautics is a very young science. From the point of view of the achievement of "heavier-than-air" flight, it is but little more than a quarter of a century since the late Professor Langley, secretary to the Smithsonian Institution at Washington, successfully flew his *model* aeroplane (he called it an "aerodrome" and it measured 12 feet from tip to tip, weighed 30lbs., and carried a steam engine and boiler weighing 7lbs.) for half-mile distances over the river Potomac. The brothers Wright achieved the first motor-driven, *man-carrying* flight in an aeroplane for a period lasting but a portion of a minute, as recently as December, 1903. Yet young as is aeronautics as a science, as an article of faith and a philosopher's dream we may claim for it the age of many centuries. Aeronautics has in fact a history, and although it is inevitable that we must accord to this history a much longer period of legend than obtains with most of the sciences, it is very real nevertheless.

Among those who have contributed effectively to the sum of ideas concerning flight no figure is more striking than Leonardo da Vinci, painter, sculptor, architect, engineer, savant, mathematician and philosopher. Leonardo, who died in 1519, has this much at least in common with the science of aeronautics—that he has only recently come into his own. Neglected for nearly three hundred years, we find the first serious discussion of his scientific manuscripts in 1797, with J. B. Venturi's "*Essai sur les ouvrages physico-mathematique de Leonardo da Vinci,*" whilst the main flood of publications both on his manuscripts and on their value in the light of modern science is a matter of living memory.

Dreamers of air conquest pass before us throughout the panorama of the early history of civilisation. We see them in the winged statuary of the Egyptians and we read of them in stories of ancient Greek mythology. Ever since man has learnt to think the challenge of the air has tantalised him. He has ever regarded as an anomaly and an incongruity that the ability to fly should be granted to birds and yet denied to him. And so we read of such legendary figures as Icarus and Perseus and Hermes, and of such entertaining but improbable stories as of Simon Magus who, with the aid of some demon colleagues, essayed a short-lived flight and finished with a broken neck; of Abaris (as related by Diodorus of Sicily) and his flight round the world on a golden arrow (somewhat reminiscent of old Mother Shipton and of the nursery pictures of the cow jumping over the moon); and of the story of Aulus Gallius in his "Attic Nights"; of Archytas and his mechanical pigeon of wood. "To wit, it was thus suspended by balancing and was animated by an occult and enclosed aura of spirit."*

Later we have the more circumstantial story of the Saracen of Constantinople who, in the presence of both the Emperor Comnenus and the Sultan of the Turks and a vast concourse of people, attempted to fly round the hippodrome at Constantinople. Wearing a long white robe braced with rods with which to catch the breeze, he took his station at the top of a tower and leaned into the wind. But "the weight of his body having more power to drag him down than the artificial wings had to sustain him, he broke his bones, and his evil plight was such that he did not long survive."†

A like fate befel a similar attempt in the year 1065 by Oliver Malmesbury.‡ Passing on, we come to Roger Bacon's classical thirteenth-century prophecy that "an instrument may be made to fly withall if one sit in the midst of the instrument, and do turn an engine, by which the wings, being artificially composed, may beat the ayre after the manner of a flying bird. . . ." No more references to flight are to be found until we reach the fifteenth century. Here we come to records concerning the famous astronomer Regiomontanus. We are told of him that in his workshop at Nuremburg was an automaton in "perpetual" motion, and that he made an artificial "fly" which, "taking its flight from his hand, would fly round the room," and at last, as if weary, would return to his master's hand, and that he fabricated an eagle which, on the Emperor's approach to the city, he sent out, high in the air, a great way to meet him, and that it kept him company to the gates of the city. Shorn of all the inevitable additions of credulous retailers of the facts, the probability is that Regiomontanus, who was of a mechanical turn of mind, fashioned a clockwork contrivance which, more by luck possibly than by design, acted as a glider when released in his workroom. Finally, there are records of experiments by an obscure contemporary of Leonardo, one Giovanni Baptista Danti, who essayed a flight at Perugia towards the close of the fifteenth century by means of a contrivance of wings which worked "with a horrible hissing sound." It was perhaps an imitation of one of the machines which Leonardo himself designed.

* Aulus Gallius, "Noctes Atticæ," Lib. X., Cap. XII.
† "Histoire de Constantinople," Cousin, Paris.
‡ "Historiæ Anglicanæ Scriptores," X., Twysden.

2 *Da Vinci and His Problem of Flight*

Leonardo da Vinci was born in 1452 and died in 1519. It is therefore obvious from what we have said that in attempting to tackle seriously the problem of flight he owed next to nothing either to his contemporaries or to those who lived before his day. Careful study of the manuscripts left by Leonardo make it clear to all fair-minded students that, in the truest sense of the term, da Vinci was not only a real pioneer of the science of flight, but he was also the first pioneer. With all his many interests and activities he yet gave, at intervals during thirty years of his life, a close consideration of the problem. It fascinated and held him. His ambition appears to have been to achieve a flight from the summit of Monte Ceceri (the name of a bird), situated a little to the north and east of Fiesole. No doubt the name appealed to the artist in him as being peculiarly appropriate. "From the mountain which bears the name of the great bird, the famous bird will take its flight, and will fill the world with its great fame,"* he writes; and again, "The great bird"—he frequently refers to his conception of a flying machine as a "bird"—"will take its first flight on the back of the great bird, and filling the world with stupor and all writings with renown and bringing glory to the nest where it was born."†

These are extravagant words which carry their own significance as to the frame of mind of him who penned them, but, coming as they do from one whom we know to have been imbued with the scientific spirit, they have an added importance. It is not surprising, therefore, that we meet with his discussions on flight both as early as the year 1490, during his residence at Milan,‡ and as late as 1514, whilst at Rome.§ His main contribution to the subject, the notebook "Sul Volo degli Uccelli" (on the flight of birds) was written at Florence in 1505.

There were certain fundamental ideas which clearly guided da Vinci through the whole course of his investigations. Of these the most important was the time-old view that the imitation of the bird was the right line to adopt and that its study would reveal the true secrets of flight. "A bird," writes he, "is an instrument working according to mathematical law, an instrument which it is within the capacity of man to reproduce with all its movements, though not with a corresponding degree of strength, for it is deficient in the power of maintaining equilibrium. We may therefore say that such an instrument constructed by man is lacking in nothing except the life of the bird, and this life must needs be supplied from that of man."‖

He had no illusions as to the comparison between a bird using its own living members and accessories and a man using wings and accessories which have no life in themselves. "The life which resides in the birds' members will, without doubt, better conform to their needs than will that of man which is separated from them, and especially in the almost imperceptible movements which preserve equilibrium. But since we see that the bird is equipped for many obvious varieties of movements, we are able from this experience to deduce that the most rudimentary of understanding and that he will to a great extent be able to provide against the destruction of that instrument of which he has himself become the living principle and the propeller."¶

Another fundamentally sound principle laid down by Leonardo should also be noted, namely, the need for study of the medium in which flight is carried out. "To attain to the true science of the movement of birds in the air it is necessary

* "Sul Volo degli Uccelli," folio 18, verso.
† "Sul Volo degli Uccelli," inside back cover.
‡ MSS. B of the Collection at the Institute of France.
§ MSS. E of the Collection at the Institute of France.
‖ "Codex Atlanticus," 161, r.a.
¶ C.A., folio 161 r.a

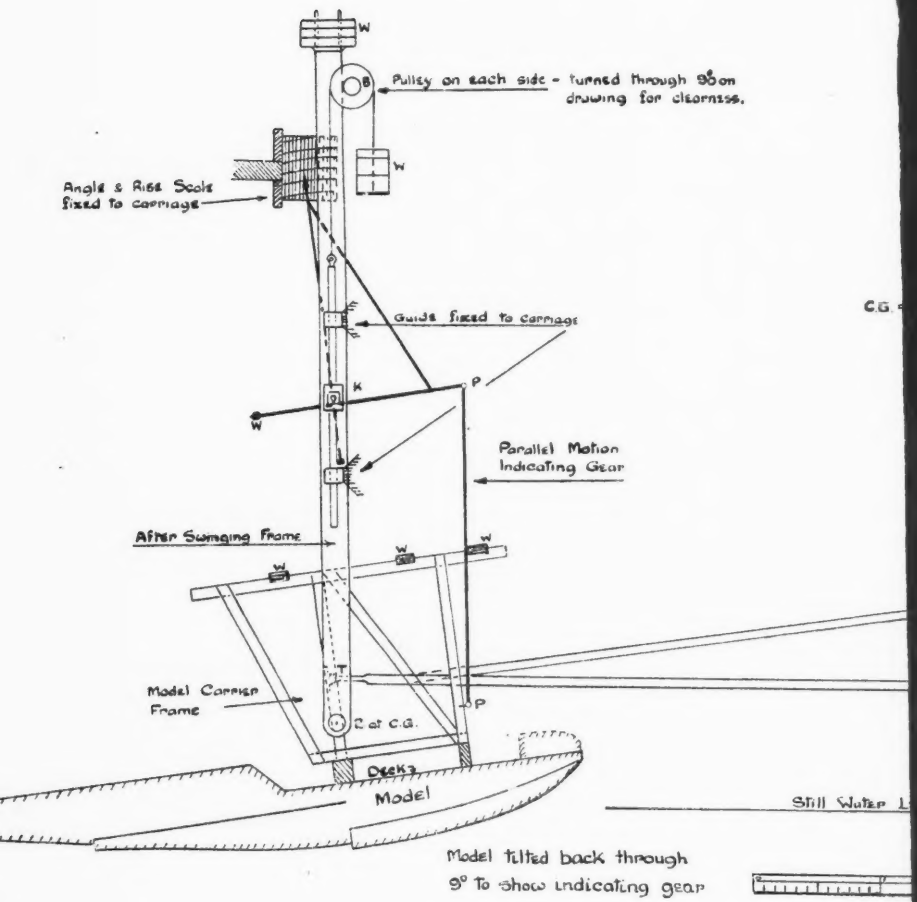
Ang
Fix

THE WILLIAM F

EXPERIMENTS WITH

APPARA

FIG. 1.

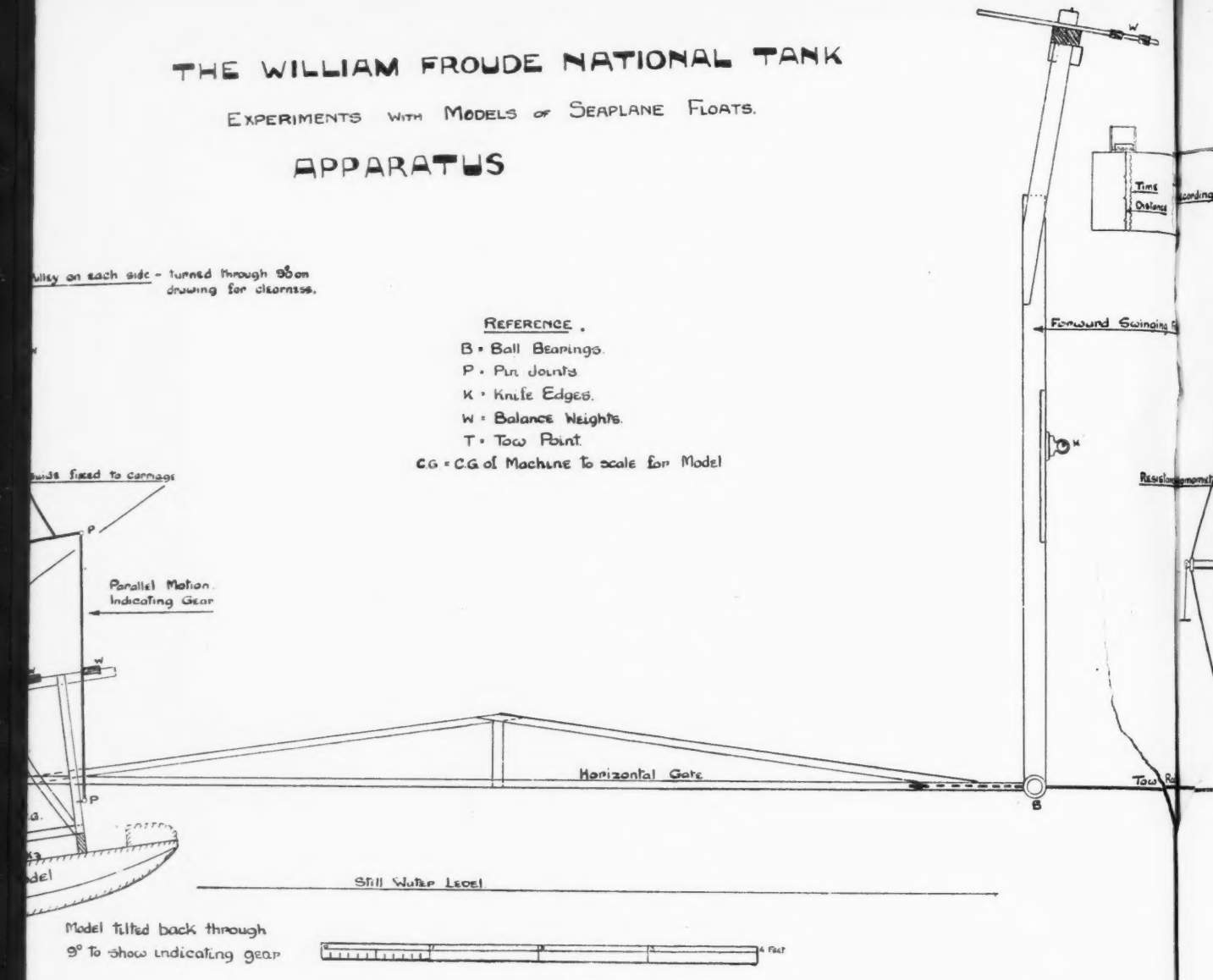


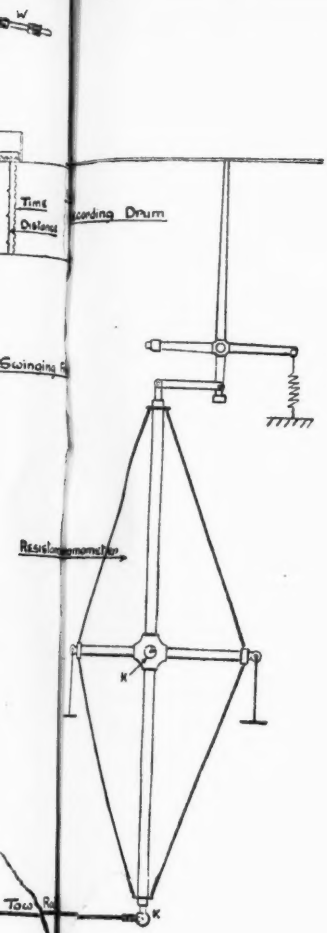
Model tilted back through 9° to show indicating gear

THE WILLIAM FROUDE NATIONAL TANK

EXPERIMENTS WITH MODELS OF SEAPLANE FLOATS.

APPARATUS


Pulley on each side - turned through 90° on drawing for clearness.
REFERENCE.
B = Ball Bearings.
P = Pin Joints
K = Knife Edges.
W = Balance Weights.
T = Tow Point.
C.G = C.G of Machine To scale for Model
Guide fixed to carriage
Parallel Motion Indicating Gear
Horizontal Gate
Still Water Level
Model tilted back through 9° to show indicating gear
Time
Distance
Forward Swinging
Tow Rope


W
Time
Distance
cording Drum
Swinging
Resistance manometer
K
K
Tow

to give first the science of the winds, which we will establish by means of the movements of water. This science will be a degree (means) of arriving at the knowledge of the winged creatures in the air and in the wind."*

Was da Vinci attempting the impossible? He lived centuries before the days of mechanical power. The problem before him was, therefore, the attachment to the human frame of contrivances to be worked by muscular energy only, in conjunction with such natural assistance as could be derived with understanding from air currents. The history of flight before the days of steam, oil and gas engines includes many similar attempts after Leonardo's times, all unsuccessful, and even up to a year or two ago we should perhaps have dismissed the idea as incapable of achievement. But the time has now come to abandon this attitude. That which Leonardo sought to achieve over four hundred years ago, and for which many an intrepid adventurer has since laid down his life, has to some extent been accomplished.

To-day aeroplane flights in machines without mechanical power, on the glider principle, have been successfully carried out. One of the consequences of the Treaty of Versailles was that Germany was forbidden to build aeroplanes fitted with engines. German inventors were thus driven to the old problem of pre-engine days, and with that superior knowledge of the principles of flight which was of necessity denied to Leonardo da Vinci, they have found a solution. This solution is not of the type for which da Vinci worked. There is no attachment to the human frame of a scheme of mechanical wings to be operated by muscular energy. It has been rather a case of a modern type of aeroplane without an engine, deriving its power from a stiffening breeze, and operated by a man, and to some extent capable of both manœuvre and control. To this extent the problem that Leonardo set himself has actually been solved.

It is important to notice that throughout Leonardo faces his problem in a true scientific spirit. He looks to those beings who do fly, he studies them carefully, their physiology, their anatomy, the medium in which they exist; he studies their movements through a vast variety of conditions. To every effect which he observes he looks for a cause, and to that cause he applies modifying conditions. He seeks and makes deductions, always with a view to obtaining control.

There are two very interesting contributions to the general subject of air conquest for which we are indebted to da Vinci that are not only important in themselves, but serve also to show another aspect of the real man of science in the object of our study. The first of these is Leonardo's invention of the parachute. In the "Codex Atlanticus" we read, "An object offers as much resistance to the air as the air does to the object." (Note here a fifteenth century statement of the law of reactions.) "You may see that the beating of its wings against the air supports a heavy eagle in the highest and rarest atmosphere, close to the sphere of elemental fire.† Again you may see the air in motion over the sea fill the swelling sails and drive heavily-laden ships. From these instances and the reasons given, a man with wings large enough and duly connected might learn to overcome the resistance of the air, and by conquering it, succeed in subjugating it and rising above it."‡ Here, clearly, we have the principle of the parachute, and Leonardo includes in his manuscript the figure here shown (Fig. 1) together with the accompanying explanation: "If a man have a tent roof of calked linen 12 braccia broad (roughly a bracci equals a yard) and twelve braccia high, he will be able to let himself fall from any great height without danger to himself."

* MSS. E, folio 54, r.

† Referring, of course, to the fifteenth century conception of the outer universe.

‡ "Codex Atlanticus," folio 372.

There is evidence that nearly a century (c. 1595) after da Vinci's invention and probably as a direct result of it, Veranzio, a Venetian, modified the original design, using a sort of square sail extended by four rods of equal size and having four cords attached at the corners.* Nevertheless, the first real descent in a parachute was not made till 1783, when Lenormand carried out a successful experiment from an observatory at Montpellier.

The other contribution is da Vinci's virtual discovery of the "lighter-than-air" principle. Leonardo was definitely familiar with the decreasing density of the atmosphere with altitude. "The air," he remarks, "has greater density when it is nearer to water and greater rarity when it is nearer to the cold region, and midway between these it is purer."† Further, he knew of the principle of the fire balloon. Vasari tells us‡ that he made figures of thin wax of strange

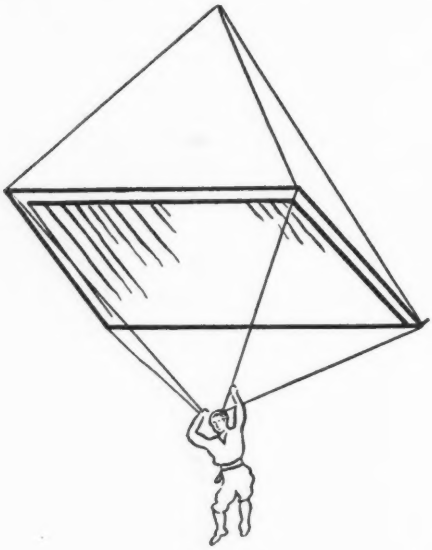


Fig. 1

shapes and filled them with warm air, causing them to "fly" through the air to the great surprise of onlookers. Why then was Leonardo not diverted to the study of balloons and aerostatics? So much simpler is this than the sister study of aerodynamics that as soon as hydrogen was discovered the science of ballooning monopolised the whole attention of students of aeronautics, to the detriment of heavier-than-air design. Having regard to da Vinci's intellect, his practical bent and his opportunities, he could undoubtedly have made the fire balloon and the parachute the starting point of a very successful venture in the field of aerostatics. But this had no appeal for him. In a balloon there is neither life nor control. In the "bird" of da Vinci's imagination there was both, and with this goal clear in his mind there was no room for diversion. We have here a clear instance of the influence of scientific purpose. One less imbued with the true scientific

* "A History of Aeronautics," Vivian, London, 1921.
† "Codex Atlanticus," folio, 161, r.a.
‡ "Lives of the Painters," Vasari, Venice, 1550.

spirit would have lost sight of the original problem in the joy of a new discovery off the beaten track. That this was not so with Leonardo is a fact which must affect our estimate of his scientific worth.

3 *Da Vinci and the Flight of Birds*

Let us now turn to some extracts from da Vinci's note-books dealing with the flight of birds. Leonardo was very clear as to the need for a proper scheme of approach to his problem. In another manuscript we read*: "I have divided the treatise on birds into four books; the first treats of their flight by beating their wings; the second treats of flight without beating their wings and with the help of the wind; the third treats of flight in general, such as that of birds, bats, fishes, animals and insects; the last of the mechanism of this movement." Again there occurs the following passage:—"Whether birds when continually descending without beating their wings will proceed a greater distance in one sustained curve. . . . Whether when they wish to pass in flight from one spot to another they will go more quickly by making impetuous, headlong movements, then rising up with reactive movement and again making a fresh descent, and

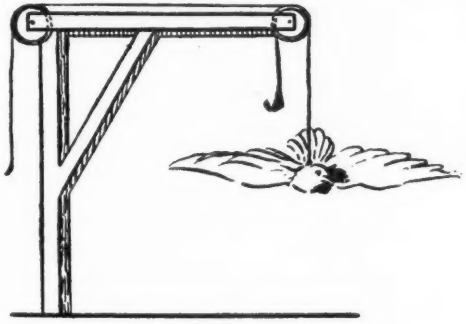


Fig 2

so continuing. To speak of this subject you must explain in the first book the nature of the resistance of the air, in the second the anatomy of the bird and of its wings, in the third the method of working of the wings in their various movements, in the fourth the power of the wings and of the tail at such time as the wings are not being moved and the wind is favourable, to serve as a guide in different movements."†

Leonardo fully recognised the rôle of centres of gravity as being fundamental in aeronautics, as in technics generally. At the outset we read in his "Flight of Birds" that "mechanical science is very noble and useful beyond all others, for by its means all animated bodies which have movement perform their operations; which movement proceeds from their centre of gravity. This is situated at the centre, except with unequal (distribution of) weight."‡ Later in the same note-book§ we meet with a most interesting little sketch of a "bird" suspended from a bracket by means of pulleys (Fig. 2), together with the note: "This is done to find the centre of gravity of the bird without which this instrument would have but little value."

* MS. K, folio 3, r.
† MS. F, folio 41, v.
‡ "Sul Volo degli Uccelli," folio 3, r.
§ Ibid., folio 16 (15), v.

Da Vinci next interests himself in the problems of wind pressure. For a bird to be sustained in the air during horizontal flight there must be an upward pressure over a moving wing surface. Accordingly he asks, " In which part of the under-surface of the width of the bird does the wing press the air more

Fig. 3

than in any other part of the length of the wings?* (See Fig. 3.) Developing his theme further, he continues: " All bodies which do not bend, whatever their size or weight, exert equal pressures on all the supports that are equi-distant from the centre of gravity, this centre being at the middle of the substance of such a body." He then considers the case of a stiff surface loaded at equal intervals with equal weights and next the case of a flexible body or surface loaded in various ways, always bearing in mind the point that " the heavier portion always guides the movement."

How is the necessary " wind pressure " required to support the bird's weight during flight to be obtained? Da Vinci finds his answer in the beating of the wings, and he studies with care the local variations in pressure in the neighbourhood of the wing surface caused by the " beating " movements. " The properties of the air are such that it may become condensed or rarified," says he. He realises clearly that support must come from air condensation below the wing. " Unless the movement of the wing which presses the air is swifter than the movement of the air so pressed, the air will not become condensed beneath the wing, and in consequence the bird will not support itself above the air. . . .

" That part of the air which is nearest to the wing which presses on it will have the greatest density."† He makes a similar point elsewhere in his notes. " When the bird shall be in the position *a n c* (Fig. 4) and shall wish to rise, it

Fig. 4

will elevate the shoulders *m* and *o*, and will thus be in the position *b m n o d*, and the air will be pressed between the sides and the point of the wings so that it will be condensed and cause an upward movement and give rise to an impetus in the air, which impetus of the air will push the bird upwards by its condensation."‡ He further develops the function of the beating of wings in this connection as follows: " The simple movement possessed by the wings of birds is

* " Sul Volo degli Ucœlli," folio 4, v.
† " Codex Atlanticus," folio 161, r.a.
‡ " Sul Volo degli Uccelli," folio 13 (12), r.

easier, as they are raised and lowered. This ease of movement is born of two causes, of which the first is that the lowering weight raises up the wings a little by themselves; the second is that the wings being convex above and concave below, the air escapes more easily the percussion of the wing with elevation than with lowering since the air in the concavity of the wing engenders more easily its condensation than its escape."* In this connection it is almost startling to notice that even that essentially modern subject of "streamlines" attracted Leonardo's penetrating attention. "That part of the air nearest the wing," he writes, "will most resemble in its movement the movement of the wing which presses on it; and that part will be more stable which is further from the said wing. That part of the air which is the nearest to the wing which presses on it will have the greatest density."†

One of the most important aspects of the "heavier-than-air" theory is that concerned with the relationship between centre of gravity and centre of pressure. In any given machine the centre of gravity is a fixed point, but the centre of pressure being dependent upon a number of varying factors, changes with the conditions of the flight. It shifts with the varying angles formed by the wing surfaces to the direction of the air opposing it, and its position can be calculated for any given position and for any given amount of wing surface from the ratio of what are termed the lift coefficient and drag coefficient respectively (in effect two components of the resultant air pressure upon the given surface) and the turning moment about some selected point. In the modern machine, longitudinal stability is assured by having the centre of gravity suitably placed and coincident with the centre of pressure and the thrust of the propeller. Obviously, however, a shift in the centre of pressure from this point will produce a "turning moment" about the new position of the point, and will disturb the equilibrium of the machine. Such changes constantly occur during flight and are necessary to effective manœuvre, but so long as they are *controlled* changes, they are all to the good.

This important factor of the centre of pressure, or centre of resistance as we may alternatively call it, was known to da Vinci, though naturally in the absence of exact mathematical knowledge he could deal with it only in a very general way. Nevertheless, his notes indicate remarkable accuracy of observation combined with scientific intuition. "When a bird, which is in equilibrium, throws the centre of resistance of the wings behind the centre of gravity, then it will descend with its head downwards. A bird which finds itself in equilibrium will have the centre of resistance of the wings more forward than the bird's centre of gravity; and such a bird will fall with its tail turned towards the earth."‡ Looking at the matter from the point of view of manœuvre, we have the following passages: "When the bird sinks, then the centre of gravity is outside the centre of its resistance; as if the centre of gravity were on the line *a b* (Fig. 5) and the centre of resistance on the line *c d*. And if the bird wishes to rise, then the centre of its gravity remains behind the centre of its resistance. As if the centre of gravity mentioned might be in *f g*, the centre of resistance would be *e h*"§ (Fig. 6). We quote yet once more from the same note-book. "The descent of a bird will be always by that end nearer to its centre of gravity. The heavier part of a bird descending will always be in front of its centre of pressure. When, without the assistance of the wind and without beating its wings, the bird remains in the air in the position of equilibrium, this shows that the centre of gravity is coincident with the centre of pressure."‖

* MS. E, folio 39, r.
† "Codex Atlanticus," folio 161, r.a.
‡ "Sul Volo degli Uccelli," folio 8 (7), v.
§ Ibid., folio 16 (15), v.
‖ Ibid., folio 8 (7), v.

4 *The Function of the Tail*

The function of the tail comes in for Leonardo's consideration and gives rise to very important conclusions. The subject affords particular interest from the point of view of scientific method. Leonardo first carries out close observations on the various uses to which a bird appears to put its tail. He makes deductions as to the functions of the tail, and then proceeds to design models in illustration of these functions. Nothing could be more sound or more essentially modern than the scientific outlook involved, quite apart from the truth or otherwise of the conclusions arrived at. It is unfortunate that comparison with the corresponding functions in the modern aeroplane is difficult, but it must be remembered that whilst in the case of the bird there is merely a tail, in the case of the modern aeroplane we have tailplane, rudder and fin, each with their separate functions. On the other hand, we must also bear in mind the interesting physiological fact that the bird, by virtue of its internal structure, can so control its air sacs as to be able at will to shift its centre of gravity within certain limits. This undoubtedly provides the bird with a mechanism for ensuring stability and control that is denied to the artificial machine.

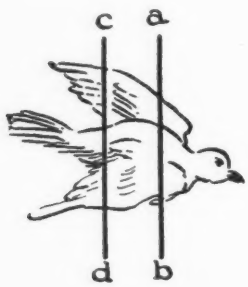


Fig. 5

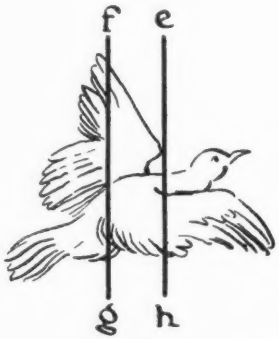


Fig. 6

We will first quote passages illustrative of da Vinci's general observations on the use of the tail in birds, *e.g.*: "The opening and lowering of the tail and the simultaneous spreading out of the wings to their full extent arrests the swift movement of birds. When birds, in descending, approach the ground and the head is below the tail, they lower the tail, spread it wide open, and take short strokes with the wings; as a consequence the head becomes higher than the tail and the speed is checked so that the bird alights on the ground without any shock."—Note here, in passing, the excellent treatment of the subject of landing, which finds so delightful a counterpart in the corresponding manœuvre in modern aviation practice.—"In all changes which birds make in their lines of movement, they spread out their tails."* On the next page of the same note-book we read, "The speed of birds is checked by the opening and spreading out of the tail."† Further observations are recorded in the "Codex Atlanticus." "Beneath the wings the down and feathers are plentiful, and at the ends of the wings and tail the tips of the feathers are flexible or capable of being bent, whilst those on the front of the wings, where it strikes the air, are firm."‡ And again, "Beginnings

* MS. L., folio 58, v.
† Ibid., folio 59, v.
‡ "Codex Atlanticus," folio 308, r.b.

of things are oft the cause of great results. Thus we may see a small almost imperceptible movement of the rudder turn a ship of marvellous size and loaded with a very heavy cargo, and that, too, amid such a weight of water as presses on its every beam, and in the teeth of the impetuous winds enveloping its mighty sails. So in those birds which can support themselves above the course of the winds without beating their wings, a slight movement of wing or tail, serving them to enter either below or above the wind, suffices to prevent their fall."*

We turn next to examples illustrative of da Vinci's combination of observation with deduction. In the note-book "Sul volo degli Uccelli" we read†: "The second shaft in the opposite portion, beyond the centre of gravity of the bird, and that is the tail, which, if it is struck by the wind underneath, since it is beyond the said centre it will cause the forward portion of the bird to lower. And if this tail be struck on top the forward portion of the bird will rise. And if this tail twist a little and turn its under surface obliquely to the right wing the anterior portion of the bird is turned to the right side. And if it turn this side obliquity of the undersurface of the tail to the left wing, it (the bird) will be turned with its forward portion toward the left side, and in each of the two manners the bird will sink.

"But if the tail, placed obliquely, be struck by the wind on its upper surface, the bird will be turned, in turning it (the tail) slowly from this side where the superior surface of the tail shows its obliquity."

Again, in manuscript K‡ occurs the following passage: "When the bird rises up by assistance of the wind without beating its wings, it spreads out and raises its wings so that they form an arch with concave side towards the sky and it receives the wind under its wings continuously, in its movements to and fro, and this would cause it to turn right over if it were not that the point of its tail is turned to the wind as it enters beneath the wind; and this afterwards by its power of resistance acts to prevent the said movement of turning over, because the wings are restrained by the tail in such a way that their various parts are of equal power, and so the tail becomes partly lowered and the bird is raised forward slightly." Perhaps one of the most interesting passages of this type is the following, which shows clearly da Vinci's interpretation of the functions of the tail with reference to that consideration of the shifting of the centre of pressure to which we have already drawn attention: "If the bird fall tail first, by throwing the tail back it will restore itself to the place of equilibrium, and if it throws it forward it will overturn itself."§

These, then, are da Vinci's observations and deductions. What did he do in the way of experiment to support them? In one manuscript‖ occurs the following highly interesting passage, affording another of those references to Leonardo's personal experiments: "Suppose that here then is a body suspended, which resembles that of a bird, and that its tail is twisted to an angle of various different degrees; you will be able by means of this to deduce a general rule as to the various twists and turns in the movements of the birds occasioned by the bending of their tails."

5 *The Movement of the Wings*

It will be convenient at this stage to consider Leonardo da Vinci's observations on the movement of the wings during "beating" flight (as distinct from "soaring" flight) and the criticisms to which these observations have been

* "Codex Atlanticus," folio 308, v.b.
† "Sul Volo degli Uccelli," folio 14 (13), v.
‡ MS. K, folio 10, v.
§ "Sul Volo degli Uccelli," folio 8 (7), v.
‖ MS. L, folio 61, v.

subjected. It will be helpful if we point out at once that the current theory as to wing movements has emerged as a consequence of the careful researches of the late J. B. Pettigrew. The results of these researches were presented in a series of memoirs* in the year 1867. We may summarise his results in his own words as follows:—

"That quadrupeds walk, and fishes swim, and insects, bats and birds fly by figure-of-eight movements.

"That the flipper of the sea bear, the swimming wing of the penguin and the wing of the insect, bat and bird, are screws *structurally* and resemble the blade of an ordinary screw propeller.

"That these organs are screws *functionally*, from their twisting and untwisting, and from their rotating in the direction of their length when they are made to oscillate.

"That they have a reciprocating action, and reverse their planes more or less completely at every stroke.

"That the wing describes a *figure-of-eight* track in space when the flying animal is artificially fixed.

"That the wing, when the flying animal is progressing at a high speed in a horizontal direction, describes a *looped* and then a *waved track*, from the fact that the figure-of-eight is gradually opened out or unravelled as the animal advances.

"That the wing acts after the manner of a kite, both during the down and up strokes."†

These conclusions apply equally to both insect and bird, but naturally the loops of the figure-of-eight will be differently proportioned in the different species. Again, during progressive flight the loops made by the wing of the insect, owing to the more oblique stroke, are more horizontal than those made by the wing of the bat and bird.

The principle is, however, in both cases the same, the loops ultimately terminating in a waved track. The impulse is communicated to the insect wing at the heavy parts of the loops *a b c d e f g h i j k l m n* of Fig. 7a; the waved tracks being indicated at *p q r s t* of the same figure. The recoil obtained from the air is represented at corresponding letters of Fig. 7b, the body of the insect being carried along the curve indicated by the dotted line. The impulse is communicated to the wing of the bat and the bird at the heavy part of the loops *a b c d e f g h i j k l m n o* of Fig. 7c, the waved track being indicated at *p s t u v w* of this figure. When the horizontal speed attained is high, the wing is successively and rapidly brought into contact with innumerable columns of undisturbed air. It consequently is a matter of indifference whether the wing is carried at high speed against undisturbed air, or whether it operates upon air travelling at high speed (as, *e.g.*, the artificial currents produced by the rapidly reciprocating action of the wing); the result is the same in both cases, inasmuch as a certain quantity of air is worked up under the wing and the necessary degree of support and progression extracted from it. It is, therefore, that as the horizontal speed of the body increases the reciprocating action of the wing decreases, and *vice versa*."‡

What then is the actual nature of the stroke of a wing during the progressive movement of a bird in horizontal flight? One further quotation from Pettigrew

* 1. "On the Various Modes of Flight in Relation to Aeronautics," J. Bell Pettigrew. Proc. Royal Inst., March, 1867.
2. "On the Mechanical Appliances by which Flight is attained in the Animal Kingdom," J. Bell Pettigrew. Trans. Linn. Soc., June, 1867.
3. "On the Psychology of Wings." Trans. Roy. Soc., Edin., August, 1867.

† "Animal Locomotion" (p. 16), J. B. Pettigrew. London, 1873.

‡ "Animal Locomotion" (p. 44), J. B. Pettigrew. London, 1873.

will serve to make this clear. "It is a condition of natural wings, and of artificial wings constructed on the principle of living wings, that when forcibly elevated or depressed, even in a strictly vertical direction, they inevitably dart forward. If, for example, the wing is suddenly depressed in a *vertical direction*, as represented at *a b* (Fig. 8), it at once darts downwards and forwards in a curve to *c*, thus converting the down stroke into a *down oblique forward stroke*. If, again, the wing be suddenly elevated in a strictly vertical direction, as at *c d*, the wing

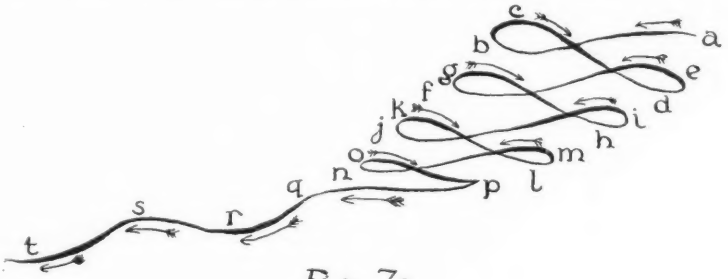


Fig. 7a

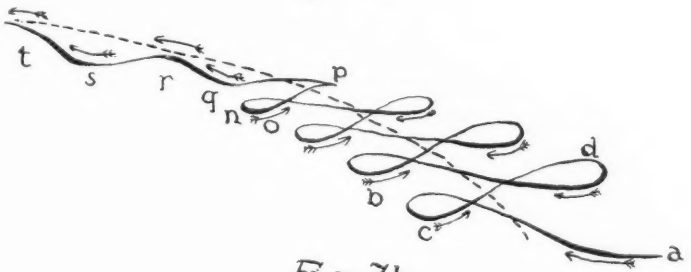


Fig. 7b

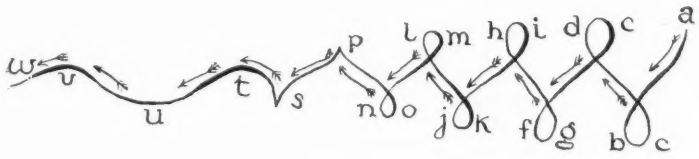


Fig. 7c

Fig. 8

as certainly darts upwards and forwards in a curve to *e*, thus converting the vertical up stroke into an *upward oblique forward stroke.*"* Clearly, then, *the wing strikes downwards and forwards during the down stroke, and upwards and forwards during the up stroke*, and this was strikingly confirmed two years after Pettigrew's statement of the theory by Marey,† who devised an instrument by means of which he was able to obtain an automatic self-registration of the wing movements for both stationary and progressive flight.

The importance of a true understanding of these movements to the student of both natural and artificial flight is beyond question. This must have been evident to Leonardo da Vinci, and references in his note-books clearly show this. It must be confessed, however, that the observations show our pioneer to be at fault. Three instances occur, one in manuscript L and two in the note-book "Sul volo degli Uccelli." In the former we read as follows: "The birds which fly swiftly, keeping at the same distance above the ground, are in the habit of beating their wings downwards and behind them, downwards to the extent necessary to prevent the bird from descending and behind when they wish to advance with greater speed."‡ In the latter we have the following passage: "If, in the descent of the bird, it rows backwards with its wings, the bird will move rapidly, and this happens because the wings strike in the air which successively flows behind the bird, to fill the void that it has left."§

These two passages clearly indicate the inaccuracy of da Vinci in the light of the facts as they have been presented by Pettigrew. The third passage shows, however, that he was not wholly wrong and that for at least a part of the movement his observations were sound. "But when the wing recovers a new force with its return *upwards and forwards*, then the big finger of the wing puts itself in a straight line with the other fingers, and thus with its cutting extremity divides the air, by some movement, high or low that the bird would rise."‖ On the whole, the balance of argument undoubtedly shows that on this subject Leonardo was at fault.

6 *Soaring Flight*

We turn now to the subject of soaring flight—that is to say, flight without the beating of wings—in so far as it was developed by Leonardo da Vinci. In this, more than in any other branch of the study of natural flight, Leonardo excelled in the care and accuracy of his observations. There are still certain problems of interest connected with the soaring flight of birds awaiting solution. It was known to Leonardo that the power of birds in keeping poised in the air for long periods with apparently very little effort is aided to some extent by the existence of varying air currents at different levels. He also knew that some of the features of the flight and buoyancy of birds are due to their instinctive knowledge and utilisation of air movements which normally appear almost too fine for detection. It is in the attempt at such detection that experiments upon the subject of soaring flight are being carried out to-day, and it is interesting to note that the possibilities have emerged largely as a result of the recent successful experiments on man-propelled machines. The spaces of the sky, in which no apparent motion is visible to the human eye, are full of a movement that the instinct of a bird can apprehend and utilise. It is known that air currents are influenced by ground obstacles, such as hills and woods, while the very colouring of the countryside, which absorbs the solar heat unevenly, gives rise to wind deflections. That da Vinci was to some extent aware of the influence of ground

* "Animal Locomotion" (p. 157), J. B. Pettigrew.
† "Revue descours scientifiques de la France et de l'Etranger," 1869.
‡ MS. L, folio 59, v.
§ "Sul Volo degli Uccelli," folio 12 (11), r.
‖ Ibid., folio 14 (13), v.

contour on air currents is evident from the following passage: "Nature has so provided that all the large birds can stay at so great an elevation that the wind which increases their flight may be of straight course and powerful. If their flight were low, among mountains where the wind goes wandering and is perpetually full of eddies and whirlwinds, they would be unable to find any spot of shelter by reason of the fury of the icy blasts among the narrow defiles of the mountains, nor would they be able to so guide themselves with their great wings as to avoid being dashed upon the cliffs and high rocks and trees, and this would sometimes prove to be the cause of their destruction."*

In 1908 the late Sir Hiram Maxim published his well-known book on "Artificial and Natural Flight," the second chapter of which was devoted to "Air Currents and the Flight of Birds." It contains no reference to Leonardo's note-book, "Sul Volo degli Uccelli." A comparison of the two books, the one written almost exactly four hundred years after the other, shows in many respects a very remarkable similarity of statements and a coincidence of views. For example, we read in Maxim, "I have often observed the flight of hawks and eagles. They seem to glide through the air with hardly any movement of their wings. Sometimes, however, they stop and hold themselves in a stationary position directly over a certain spot, carefully watching something on the earth immediately below. In such cases they often work their wings with great rapidity, evidently expending an enormous amount of energy. When, however, they cease to hover and commence to move again through the air, they appear to keep themselves at the same height with an almost imperceptible expenditure of power."† And again, later, we meet with the following passage: "I have often noticed that gulls are able to follow a ship without any apparent exertion; they simply balance themselves on an ascending column of air, where they seem to be quite as much at ease as they would have been roosting on a solid support. If, however, they are driven out of their position, they generally commence at once to work their passage. If anything is thrown overboard which is too heavy for them to lift, the ship soon leaves them behind, and in order to catch up with it again they move their wings very much as other birds do; but when once established in the ascending column of air, they manage to keep up with the ship by doing little or no work. In a calm or head wind we find them directly aft of the ship; if the wind is from the port side they may always be found on the starboard quarter, and *vice versa*."‡ Again, later in the chapter we read of "soaring birds which practically live upon the wing and, by some very delicate sense of touch, are able to feel the exact condition of the air . . . and I have no doubt that the air cells, which are known to be very numerous and to abound throughout the bodies of birds, are so sensitive as to enable soaring birds to know at once whether they are in an ascending or a descending column of air."§

It is obvious here that the contention is that soaring birds are capable of periods of "rest" by supporting themselves on those columns of ascending air which are so plentifully produced in nature by the ordinary processes of convection. One further quotation will serve to complete the summary of the position and to enable us to make comparisons with the corresponding entries in Leonardo da Vinci's note-books. "Suppose," says Maxim, "that the local influence which causes the up and down motion of the air should be sufficiently great to cause the air to rise at the rate of two miles an hour, and that the wind at the same time should be blowing at the rate of ten miles an hour, the motion of the air would then be the resultant of these two velocities. In other words, it would be blowing up an incline of 1 in 5. Suppose now that a bird should be able to so

* "Codex Atlanticus," folio 308, v.b.
† "Artificial and Natural Flight," p. 13. Maxim, London, 1908.
‡ Ibid., p. 20.
§ Ibid., p. 23.

adjust its wings that it advanced five miles in falling one mile through a perfectly calm atmosphere, it would then be able to sustain itself in an inclined wind, such as I have described, without any movement at all of its wings. If it were possible to adjust its wings in such a manner that it could advance six miles by falling through one mile of air, it would then be able to sustain itself in an inclined wind, such as I have described, without any movement at all of its wings. If it were possible to adjust its wings in such a manner that it could advance six miles by falling through one mile of air, it would then be able to rise as relates to the earth, while in reality falling as relates to the surrounding air."*

We turn now to compare these statements with the following passages from da Vinci's note-books: "The imperceptible flutterings of the wings without any actual stroke keeps the bird poised and motionless amid the moving air."† "The kite and other birds that move their wings very little seek the air currents and when the wind is blowing high up then they will be seen at a great height and if the wind blows low down they remain low down." "When there is little wind the kite beats several times with its wings during flight so that it may rise and obtain impetus, with which impetus descending a little, it goes a long distance without beating its wings; and when it has dropped somewhat it repeats the movements and so successively; and this drop without beating the wings serves as a means of repose in the air after the fatigue of the said beating of the wings."‡ And again, "The helms which are on the shoulders of the wings are necessary when the bird in its flight without beating its wings wishes to maintain itself in part of a tract of air, upon which it is either slipping down or rising, and when it wishes to bend either upwards or downwards or to right or left."§

The similarity of treatment with Maxim is strikingly indicated, too, by the following further passages from the manuscript "On the Flight of Birds": "Here the big fingers of the wings are those which hold the bird still in the air against the movement of the wind; that is to say, the wind moves on which it supports itself without beating its wings and the bird does not change its place. The reason of this is that the bird arranges its wings on such an obliquity that the wind, which strikes it underneath, does not cause a wedge of such a nature as would lift it up, but lifts it up however just as much as its weight would press it down; that is to say, if the bird fall with a force of two the wind would lift it up with another force of two and thus . . . the bird remains in its place without rising or falling."‖ Later in the same note-book we meet with the following passage: "Let us say, that the impetus be as 6, and that the bird weigh 6, and that in the middle of the movement the impetus become 3, and that the weight still remains 6; here the bird would sink by half-movement, that is to say, by the diameter of the square and the wing oblique with the contrary aspect, also by the diameter of this square does not allow such a weight to sink, neither does the weight permit the bird to rise; consequently it moves in a straight line. That is to say, the descent of the bird during the said half-movement would be by the line *a b* "¶ (Fig. 9).

No study of the soaring of birds can be considered in any sense complete without due treatment of the large circular sweeping movements which form so familiar a spectacle of the skies, and these happily come in for a large share of attention in Leonardo's note-books. Thus, in the "Codex Atlanticus" we read: "The ways in which birds rise, without beating their wings but by circles, with the help of the wind, are of two kinds—simple and complex. The simple comprise those in which in their advancing movement they travel above the wind, receiving

* "Artificial and Natural Flight," p. 19. Maxim.
† "Codex Atlanticus," folio 308, v.b.
‡ "Sul Volo degli Uccelli," folio 6 (5), v.
§ MS. K, folio 7, v.
‖ "Sul Volo degli Uccelli," folio 14 (13), r.
¶ Ibid., folio 15 (14), v.

its buffetting from beneath, and so finish the reverse movement against the wind. The complex movement by which birds rise is also circular, and consists of an advancing and reverse movement against the direction of the wind in a course which takes the form of a half circle, and of an advancing and reverse movement which follows the course of the wind.

"The simple circular movement of rising without beating the wings will always occur when there is great agitation of the winds, and this being the case, it follows that the bird in so rising is also carried a considerable distance by the force of the wind. And the complex movement will be found to occur when there are light winds, for experience shows that in these complex movements the

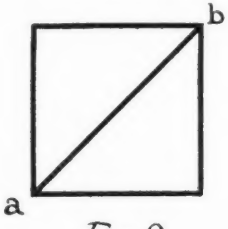


Fig. 9

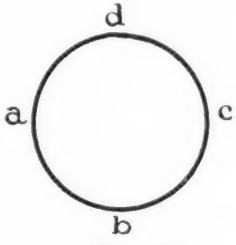


Fig. 10

bird rises through the air without being carried too far by the wind in the direction in which it is travelling."* In the manuscript "Sul Volo degli Uccelli" this theme is further developed with minute completeness,† in the course of which we meet with the following summary: "I conclude that the mounting of the bird without the beating of wings is caused by nothing other than its circular movement which, when it starts from the arrival of the wind, sinks until it reaches the place where the reflex movement begins, after which and so circulating, it has described a semi-circle and its face turned to the wind, and follows the reflex movement on the wind still circulating until, with the help of the wind, it makes its greatest height between its lowest and the arrival of the wind and is left with the left wing to the wind; and from this greatest height again circulating, it descends to the last incident movement, being left with the right wing to the wind. As if to say, the wind goes from *a* to *c* (Fig. 10) and the bird moves from *a* and sinks from *a b c* and in *c* it makes the reflex movement as in *c d a* and by the favour of the wind it is much higher at the end of the reflex movement, which end of the reflex movement is started perpendicularly over the said commencement of the incident movement."‡

7 *Manœuvre*

We may now turn with profit to the question of manœuvre and control, a subject to which da Vinci gave the closest attention. He went far beyond the stage of mere observation. As befits a man with an essentially scientific outlook, he considered practically every type of manœuvre for which one would look in an artificial machine; he closely observed the corresponding procedure in the case of birds and then brought scientific principles to bear upon their physical explanation. In connection with the main topics so far considered in this paper, as for example in the section on the functions of the tail, we have of necessity already

* "Codex Atlanticus," folio 308, r.b.
† "Sul Volo degli Uccelli," folio 7 (6), r; folio 13 (12), r and v; folio 15 (14), v.
‡ Ibid., folio 15 (14), r.

met with a number of references of this kind. The examples are nevertheless by no means exhausted, and we now proceed to indicate some others. As an instance, let us consider the manœuvre of turning. The necessity for "banking" was well known to da Vinci, who tells us that when the bird wishes to turn to the right or the left by beating its wings then it will beat lower with the wing on the side to which it wishes to turn (Fig. 11), and thus the bird will turn the movement

Fig. 11

behind the impetus (elan) of the wing which moves most (Vuscello (si) torcera il moto direto all impeto dell'alia che piu si mosse)."*

Other observations on this manœuvre in birds are as follows: "The bird when it wishes to turn does not beat its wings with equal movement, but moves the one which makes the convex of the circle it describes more than that which makes the concave of the circle."† And further on in the same manuscript, "The bird beats its wings repeatedly on one side only when it wishes to turn round while one wing is held stationary; and this it does by taking a stroke with the wing in the direction of the tail, like a man rowing in a boat with two oars, who takes many strokes on that side from which he wishes to escape, and keeps the other fixed,"‡ whilst the case of rapid turning in a high wind is dealt with as follows: "When the bird is carried along by the wind and wishes to turn quickly towards it, it will then enter beneath the wind with the wing turned towards it; and then with the feathers of the tail turned towards the wind, it will enter upon it, and so by the help of the wind striking upon its tail it turns much more rapidly."§

There is yet one further reference to the manœuvre of sudden turning to be found in the note-book "On the Flight of Birds." This calls for special attention on account of the clear indication it contains as to Leonardo's acquaintanceship with the principle of inertia. Two facts in this connection should be carefully borne in mind. Firstly, that in the fifteenth century the science of dynamics had hardly been born; and secondly, that according to accepted histories of mechanics,‖ the so-called law of inertia was discovered by Galileo, and was presented by him in his "*Discorsi e demonstrazioni matematiche*" in 1638. In the light of these facts the following entry in Leonardo da Vinci's note-book is most remarkable. "When a bird wishes to turn itself suddenly on one of its sides, then it pushes quickly the point of the wing on this side towards its tail (Fig. 12), and *because all movement tends to maintenance*, or rather, *all moved bodies continue to move as long as the impression of the force of their motors remains in them*, the movement then of such a wing thrust with violence towards the tail reserving still at the end a part of the said impression, not being able by itself to follow the movement commenced at first, will move with itself all the bird, until the impetus of the moved air may be consumed. The tail pushes with its face, and the wind struck by it, makes the bird move suddenly in the contrary direction."¶ Turning to the manœuvre of rising, we have the following interesting paragraph:

* "Sul Volo degli Uccelli," folio 6 (5), r.
† MS. K, folio 4, v.
‡ MS. K, folio 7, r.
§ MS. K, folio 9, v.
‖ *e.g.*, "Science of Mechanics," E. Mach. English edition, p. 141.
¶ "Sul Volo degli Uccelli," folio 13 (12), r.

"When the bird by the beating of its wing wishes to rise, it raises the shoulders and it beats the points of the wings towards itself, and so condenses the air which lies between the points of the wings and the breasts. The tension (of the condensed air) lifts up the bird."* This is elsewhere developed further from the point of view of stability. "When the bird rises up by assistance of the wind without beating its wings, it spreads out and raises its wings to form an arch with the concave side towards the sky, and it receives the wind under its wings continually in its movements to and fro. This would cause it to turn

Fig. 12

right over if it were not that the point of its tail is turned to the wind as it enters beneath the wing, and this afterwards by the power of resistance acts to prevent the said movement of turning over, because the wings are restrained by the tail in such a way that their various parts are of equal power, and so the tail becomes partly lowered and the bird is raised forward slightly."†

One of the most interesting features of the note-book "On the Flight of Birds" is the series of passages which deal with problems of stability and control under variety of conditions. Leonardo dicsusses, for example, the behaviour of birds on meeting with a gust of wind. "If the point of the wing be struck by the wind, and this wind enter under such a point (Fig. 13) then the bird would

Fig. 13

be liable to be upset unless it uses one of two remedies, that is to say, either that it force suddenly such a point under the wind or that it lower the opposite wing, from the middle on."‡ This latter alternative, in effect, is designed to create a

* "Sul Volo degli Uccelli," folio 6 (5), v.
† MS. K, folio 10, v.
‡ "Sul Volo degli Uccelli," folio 7 (6), v.

restoring moment. Leonardo frequently makes use of the varying extent of wing surface presented to a wind as a method of maintaining control and equilibrium. The following passage is a striking illustration of this: " When the wind strikes the bird under its course from its centre of gravity towards this wing (Fig. 14) then such a bird will turn itself with its spine to the wind, and

Fig. 14

if the wind were more powerful below than above then the bird would turn upside down, *if it did not immediately take care to draw in the under wing and stretch out the over wings. In this manner it rights itself and returns to the position of equilibrium.* The proof is thus: Let *a c* (Fig. 15) be the wing with-

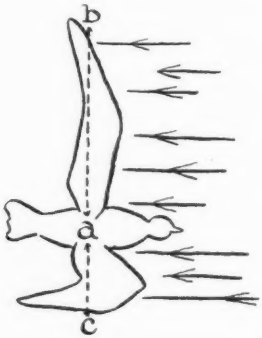


Fig. 15

drawn under the bird, and *a b* be the extended wing. I say that *the forces of the wind that strike the two wings will have the same proportion as that of their extensions,* that is to say, *a b* to *a c*. It is true that *c* is wider than *b*; but it is so near the centre of gravity of the bird that it offers little resistance in comparison with *b*."*

In the same note-book there is to be found an illuminating little passage worthy of notice in this connection, since it shows how sincerely scientific was Leonardo's treatment of problems of wing span in relation to wind pressure. " That the wing does not utilise all the pressure of the air," says he, " and that this is true, note that the interstices of the primary feather are spaces much wider than the width of the feathers themselves; therefore you who study flying should not calculate on the entire surface of the wing, but note the different varieties of wings for all flying creatures."† In thus drawing a clear distinction between *apparent* wing surface and *effective* wing surface, he gives us clear

* " Sul Volo degli Uccelli," folio 10 (9), r.
† Ibid., folio 9 (8), v.

evidence of a true appreciation of values. He was able in fact to probe beneath the surface of his problem.

In manuscript L there occurs an interesting example dealing with "head gusts." "When the wind is about to throw the bird backwards then the bird draws together the shoulders of its wings, so that its weight is massed more to the front than it was at first, and consequently the part that is heaviest is first in its descent while in addition the tail is spread out and bent down."* The following passage is also interesting as being typical of da Vinci's general treatment of problems of equilibrium during flight: "If the wing and the tail are too much on the wind, lower half the opposite wing, and therewith receive the force of the wind and equilibrium will be restored.

"And if the wing and the tail were under the wind raise the opposite wing and it will be corrected to your desire, provided that such wing as is raised be less oblique than that which is opposite it.

"And if the wing and the breast are on the wind, one must depress half the opposite wing, which will be struck by the wind and forced upwards, which will right itself.

"And if the bird is so that its hind quarters are on the wind then the tail ought to be forced under the wind, and thus one will be able to equalise the powers.

"But if the bird has its hind quarters under the wind let it enter with the tail on the wind, and it will right itself."†

8 *Safety Precautions in Natural and Artificial Flight*

We next turn to examples of Leonardo da Vinci's comments on the subject of safety precautions in flight. He had no illusions as to the dangers attendant upon attempts at artificial flight. Consequently it is no matter for surprise that references to safety devices, both in the design of apparatus and in the governing principles of control, are to be found fairly plentifully in his note-books. In particular he clearly realised that which modern aviators universally regard as almost a first principle in safe flying, namely, maintaining as high an altitude as possible. There are very few accidents to apparatus, or troubles due to sudden gusts, resulting in the temporary upsetting of the equilibrium of a machine, which cannot be corrected, given sufficient time and manœuvring space. The restoration of control, in the machine of to-day, is largely a matter of skilful pilotage. But this restoration does take time, and if during this period the machine is falling then disaster must ensue unless a high altitude had at first been attained. "High flying is safe flying" is the accepted principle to-day, and it was clearly so accepted by Leonardo da Vinci four hundred years ago. In his "Sul Volo degli Uccelli" we read: "The movement of the bird ought always to be above the clouds lest the wing should be moistened to disclose a broader view and to avoid the peril of wind revolutions among mountain gorges, which are always full of whirlings and turnings of the winds. Beyond that, if the bird should turn somersault, you would have more time for righting it, with the directions already given, before it reaches the ground."‡

In the next page of the same note-book Leonardo brings this point out again, more specifically with respect to artificial flight, and he deduces the best materials advisable for the construction of the artificial limbs. He condemns the use of iron fittings and argues in favour of joints of strong tanned leather with "nerves" or "sinews" of raw silk cord. The note is illustrated by a "limb" or wing, and reads as follows: "The said bird ought, with the assistance of the wind, to rise to a great height, and that will be its safety; even should it experience all the

* MS. L, folio 62, r.
† "Sul Volo degli Uccelli," folio 9 (8), r.
‡ Ibid., folio 7 (6), v.

above-mentioned revolutions, it has time enough to recover its balance provided that its limbs be very strong so that they may safely resist the fury and vigour of the descent with the above-mentioned defences, its joints of strong tanned leather and its nerves of raw silk cord of great strength; and let not anyone hamper himself with fittings of iron, because they burst easily in twisting, or waste away, for which reason they are not to be used."* On the assumption—and with Leonardo this amounted almost to an article of faith—that man is capable of flight, he wisely advises as much freedom from encumbrances as possible. "A man with wings should," he writes, "be free from the waist upwards in order to balance himself as he does in a boat in order that his centre of gravity and that of the instrument might be able to balance and change when necessity required it, according to the change in the centre of its resistance."† Fig. 16 shows in a modified form his diagram in illustration of this point.

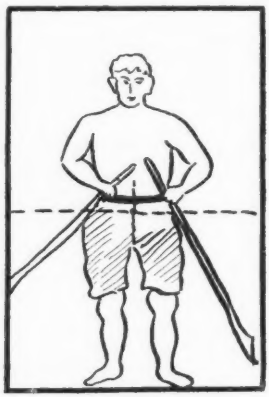


Fig 16

Another interesting passage, later on in the same note-book, is headed "To escape the peril of ruin" and reads as follows: "The ruin of such instruments may happen in two ways, of which the first is that the instrument break; the second is that the instrument turn on its edge or near its edge, because it ought always to descend by a great obliquity or almost by the line of equality. . . . As to the protection of the instrument from rupture, one avoids it by making it of the greatest strength by no matter what line it may be able to turn itself, that is to say, either by the edge falling either with the head or the tail first, or even with the point of the right or left wing, or by the halves or quarters of the said lines as the design shows (referring to a figure accompanying the note). As to turning by any side whatever of the edge, one ought to prevent it even in the beginning by making the instrument in such a fashion that in descending under whatever aspect that may come about the prevention should be ready; and this will be done in giving its centre of gravity on the centre of the weight carried by it, always in a straight line, and one of its centres very distant from the other; that is to say, that in an instrument 30 braccia wide these centres may be 4 braccia apart and that one, as mentioned, be situated under the other and the

* "Sul Volo degli Uccelli," folio 8 (7), r.
† Ibid., folio 6 (5), r.

heavier underneath, so that in the descent the heavier portion might always be the guiding portion of the movement."*

Finally, there is one further note hidden away in the corner of a page in which da Vinci, in anticipation of a fall by the fifteenth century airman, provides the means for absorbing the shock of collision with the ground. Referring to a sketch of some wine skins strung together, he writes: "Bags, where a man, falling from a height of 6 braccia would not injure himself, falling either in water or on land; and that these bags, fastened together in the fashion of beads, are surrounded by others."† On the reverse side of this same folio is the comment, "If you fall, see that you strike the ground with the double wine skin that you hold under you."‡

9 *Leonardo da Vinci's Flying Machines*

We come finally to da Vinci's notes on flying machines. We have seen the general nature of our philosopher's observations and experiments and of the arguments and principles he brought to bear upon them. How did he apply these to the design of suitable mechanism wherewith to achieve for mankind his ambition of mastery over the air? He lay down very specifically a principle for general guidance in the design of wings. "You are to remember," wrote he, "that your bird (*i.e.*, flying machine) ought not to imitate anything but the bat (Fig. 17), because the membranes form an armour or liaison to the armour, that

Fig. 17

is to say, strength to the wings. And if you imitate the wings of the feathered birds, the wings are more powerful in bone and nerve, through being pervious; that is to say, the feathers are disunited and permeable to the air. But the bat is aided by the membrane which binds the whole and is not pervious."§ This was sound advice and finds its final expression in modern aviation design. It is but a small step from the impervious membrane of a bat's wing to the doped fabric of an aeroplane wing. Yet how far did Leonardo follow this advice himself? Here was a line of attack on his practical problem which, in the light of modern developments, would appear to have contained the germs of some possible success. Yet in fact he appears to have neglected it, and it is disappointing that the sketches and notes in his manuscripts show Leonardo occupied chiefly with the idea of the substitution of "jointed" oars for wings. An almost isolated exception occurs in one note-book in which we meet with a sketch of an artificial wing (Fig. 18), obviously modelled after the wing of a bat. Against the shaded portion *X* he writes: "Make the meshes of this fibre of $\frac{1}{8}$in. width." Further details of construction are given with reference to the letters *A*, *B* and *C* in the

* "Sul Volo degli Uccelli," folio 13 (12), v.
† Ibid., folio 17 (16), r.
‡ Ibid., folio 17 (16), v.
§ Ibid., folio 16 (15), r.

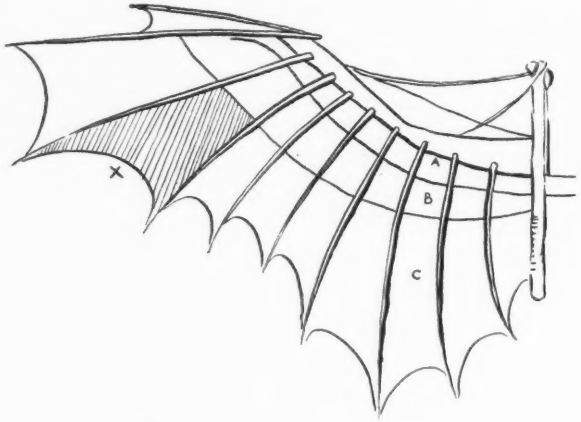


Fig. 18

diagram as follows: "*A* will be of blades of deal, which has threads (?) and is light. *B* will be of fustian, on which will be glued the feathers, so as not to be easily pervious to the air. *C* should be of starched light silk, and in order to test it, you may make it of thin pasteboard."*

Presumably Leonardo abandoned this type of wing as impracticable. Possibly he may have concluded that the wind resistance created by such an expanse of wing surface was far too much for the muscular energy of the human frame to cope with, but if this had been the case, one would have supposed the scientific alternative would have been to have reduced the area of surface accordingly. It seems probable that Leonardo attempted some experiments with a mechanical wing, and if so it is difficult to believe that the relationship between wing surface and wind resistance did not claim his attention. Thus we meet with a sketch of a man operating a mechanical wing (Fig. 19) accompanied by

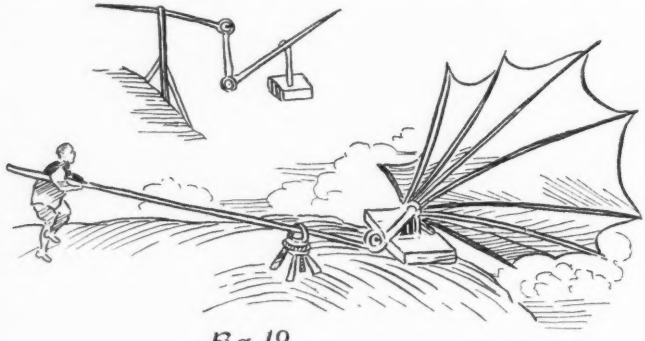


Fig 19

the following explanatory note: "If you wish to see the true test of the wings, take some pasteboard strengthened with fibre, and fitted with cane ribs, a wing

* MS. B, folio 74, r.

of the width and length of 20 braccia at least, and fix it on a board (sheet-pile) of 200 pounds weight;* it will produce, as shown in the figure, an effective force. And if the board of 200lbs. is lifted before the wing falls, the test is good, but see to it also that the force be prompt (to act) and if the above-mentioned effect is not obtained, lose no more time on it."†

We conclude from Leonardo's note-books—and these are almost our sole materials for judgment—not only that he either abandoned or neglected the idea of a "bat's wing" surface, but also that he came to no settled conclusions as to the use of a "jointed oar." Regarding it from the modern point of view, it is obvious that the air displacement created by the movement of a form of "oar" through the air must of necessity be utterly inadequate for the generation of a sufficient force to support a human being plus mechanical attachments. Consequently one need not be surprised at the variety, the vagueness, and the incompleteness of design displayed in many sketches and notes in manuscript B

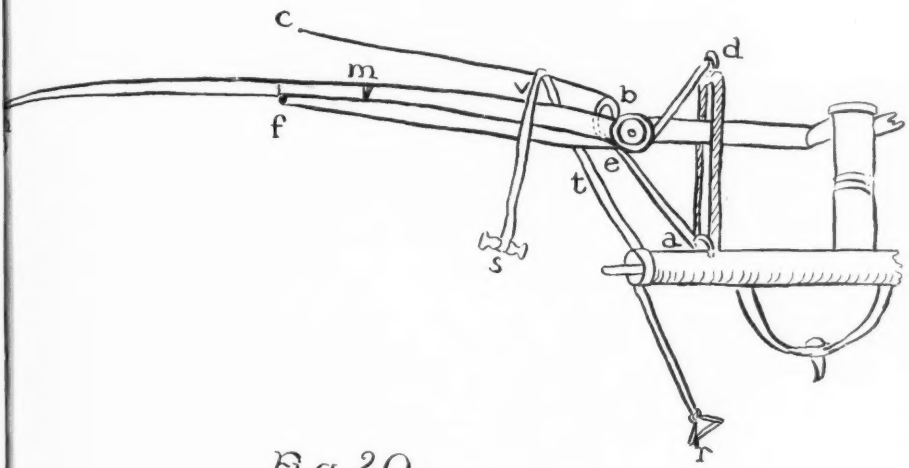


Fig 20

and in the "Sul Volo degli Uccelli." They reflect the artist and the visionary vainly striving to link up with the engineer and the scientist. Da Vinci's scientific intuition was wonderful. His "sensing" of laws of nature was one of the most remarkable traits in his personality, and it carried him far—certainly far beyond his contemporaries. But there are serious limitations to this type of personality. Between the sensing of a law and an exact knowledge of its comprehension and its consequences lies a wide gulf of mathematical expression and equipment, and in this gulf it was inevitable that Leonardo would grope and flounder with little chance of success.

As a consequence the flying machines of Leonardo da Vinci are mainly of artistic and historic interest. Fig. 20 shows a typical example of a da Vinci wing—the left half only of the "machine." The following note accompanies the sketch: "*a b c* arranges that in rising the part *m n* is promptly raised; *d e f* arranges that in descending *m n* is promptly lowered, and the wing (thus) fulfils

* Sketches explanatory of these parts are shown in MS. B, folio 77, v.
† MS. B, folio 88, v.

its purpose; *r t* lowers the wing with the foot, that is to say, by extending the legs; *v s* raises the wing by the hand and the turn."*

Fig. 21 shows one of da Vinci's flying machines. The aviator is lying on a plank in front of which is a sort of pole. Fixed to this pole we see a rounded iron shank, and the jointed "oars" or wings are attached to this shank (Fig. 21). The wings are operated by the feet by means of stirrups *c* and *d*, the right foot lowering the wing and the left raising it. The design is difficult to follow, but that Leonardo intended it seriously is evidenced by the following interesting paragraph: "You will experiment with this instrument on a lake, and you will carry engirdled a long wine skin, so that in falling you will come to no harm."†

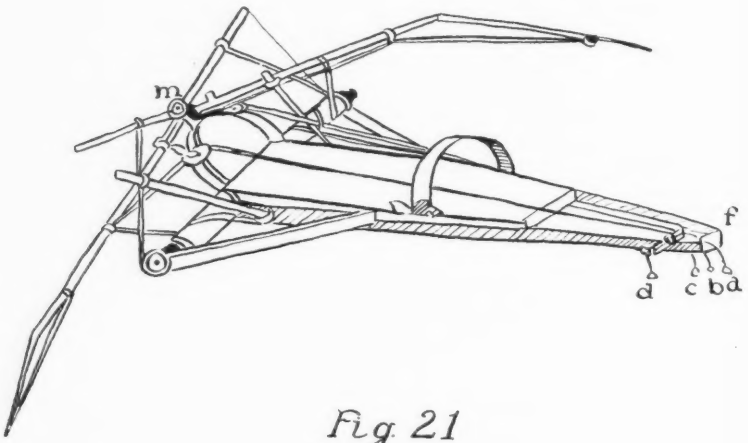


Fig. 21

Other designs‡ show double systems of oars, and he frequently favours the use of two ladders at the base of the machine to represent the feet of the "bird" and to serve the purpose of climbing into his instrument.

There is, however, one further design which calls for comment, since it constitutes a very real contribution to the science of aviation. It shows, in fact, that da Vinci may fairly lay claim to the invention of the helicopter; that is to say, to a type of machine capable of vertical movement upwards and downwards, and of hovering in any given position for any required time. Leonardo's sketch is outlined in Fig. 22. It consisted of a mechanism furnished with a helical wheel. This wheel is actuated by a twisted spring which, when released so as to unwind itself, sets the helical wheel in rapid rotation. Referring to this instrument, Leonardo comments, "I say that if this instrument made with a helix is well made, that is to say, of flaxen linen, of which one has closed the pores with starch, and is turned with great speed, the said helix is able to make a screw in the air, and to climb high. Take the example of a wide and thin ruler and directed violently into the air; you will see that your arm will be guided by the line of the edge of the said board. . . . One is able to make a little model of this of cardboard, whose axis should be of thin sheet iron, twisted with force;

* MS. B, folio 73, v.
† MS. B, folio 74, v.
‡ *e.g.*, MS. B, folio 80, r.

on freeing this, it causes the helix to turn."* Here then, beyond doubt, and for the first time in history, we have the principle of the helicopter.

What are we to conclude from all these activities? We have reviewed at some length the full scope of Leonardo da Vinci's researches and investigations in the field of aviation, and it is impossible to withhold our admiration for their breadth and their thoroughness. That he failed to achieve flight in no wise detracts from the value of his work. It is doubtful indeed if he ever even made the attempt himself. Jerome Cardan, the mathematician, whose father was a contemporary of Leonardo, who knew him and his work, tells us in his

Fig. 22

"de Subtilitate" that "Leonardo da Vinci also attempted to fly, but misfortune befel him from it. He was a great painter." This appears to be the only reference to an actual attempt. Nevertheless, Leonardo's work in aviation was real enough, and having regard to the limitations imposed upon him by the knowledge of his days, it was *scientific*. At all times a student of nature and her mysteries, and gifted as he was with a bent for observation and deduction, he was able to combine with these qualities his mechanical genius as an engineer and his wonderful imagination as an artist. It is unfortunate that his manuscripts were scattered and lost to the world for so long.

(To be continued.)

* MS. B, folio 83, v.

ROYAL AERONAUTICAL SOCIETY

(Founded 1866).

Patrons: H.R.H. the Prince of Wales, K.G., Group Captain H.R.H. the Duke of York, K.G., R.A.F.

President: Lord Weir of Eastwood.

Vice-President: Major-General Sir R. M. Ruck, K.B.E., C.B., C.M.G.

Chairman: Professor L. Bairstow, C.B.E., F.R.S.

Vice-Chairman: Lieut.-Colonel Mervyn O'Gorman, C.B., D.Sc.

Council

Brigadier-General R. K. Bagnall-Wild, C.M.G., C.B.E.
Griffith Brewer, Esq.
Wing Commander Cave-Browne-Cave, C.B.E.
Sir Mackenzie D. Chalmers, K.C.B., C.S.I.
C. R. Fairey, Esq., C.B.E.
H. P. Folland, Esq.
Squadron-Leader R. M. Hill, M.C., A.F.C.
Professor C. Frewen Jenkin, C.B.E.
Professor B. Melvill Jones, A.F.C.
Lieut.-Colonel J. T. C. Moore-Brabazon, M.C., M.P.
J. D. North, Esq.
Colonel A. Ogilvie, C.B.E.
Professor A. J. Sutton Pippard, D.Sc.
J. L. Pritchard, Esq.
Col. the Master of Sempill, A.F.C.
Major R. V. Southwell.
Lieut.-Colonel H. T. Tizard, A.F.C.
Major H. E. Wimperis, O.B.E.

Honorary Treasurer: Mr. A. E. Turner.

Secretary: Lieut.-Colonel W. Lockwood Marsh (late R.A.F.).

Offices: 7, Albemarle Street, London, W.1.

Aims

The Society exists for the furtherance of the Science of Aeronautics, and its activities fall into the following headings:—(1) Protecting the interests of the aeronautical profession by conferring a technical status on those qualified for such distinction. It acts as the professional society or institution of qualified aeronautical engineers. (2) Organising discussions and publishing papers on subjects of importance in connection with the various branches of aeronautical science. (3) Encouraging and assisting technical students who desire to adopt the aeronautical profession for their careers. (4) Providing an organisation wherein those interested in aeronautics from scientific or other motives, but who are not professionally connected with aviation, may meet together, have opportunities of study and keep themselves in touch with aeronautical affairs.

Membership

The membership is divided into two categories:—

(*a*) *Technical*—(1) *Students*: Reserved for those under the age of 26 who are receiving a technical training such as will fit them in due course to become Associate Fellows. No entrance fee; *Subscription*, 1 guinea.

(2) *Associate Fellows*: Reserved for those who are duly qualified in accordance with the Regulations. Entitled to use the letters A.F.R.Ae.S. after their names. *Entrance fee*, 3 guineas; *Subscription*, 4 guineas.

(3) *Fellows*: Reserved for those who have attained to considerable eminence in the science of aeronautics, and are duly qualified in accordance with the Regulations. Entitled to use the letters F.R.Ae.S. after their names. *Entrance fee*, 5 guineas; *Subscription*, 6 guineas.

(*b*) *Non-technical* (open to all).—*Members*: All who desire to support the Society are eligible. *Entrance fee*, 1 guinea; *Subscription*, 4 guineas.
Foreign Members: Ditto. Domiciled abroad. No entrance fee; *Subscription*, 2 guineas.
Associate Members Ditto. No entrance fee; *Subscription*, 2 guineas.

Privileges

Members in any of the above grades are entitled to attend the Society's lectures, receive the monthly Journal free of charge and consult books in the Society's library. All grades except Associate Members are entitled to borrow books from the library. Students and Associate Members are not entitled to vote.